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SCIENTIFIC ENTITIES II

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IT was suggested in the first part of this paper* that much scientific thought rests upon a logical dualism: upon an assumption that some of the things with which the scientist deals have a different kind of *being* from others of the things with which he deals, and that some of the propositions with which he is concerned have a special kind of *truth*. In particular many scientists have accepted an assumption that "making an inventory of things or events" (describing) is logically different from "making things or events intelligible" (explaining), and have assumed that things or events are made intelligible when they are shown to be, despite appearances, necessary. In many scientific theories special entities are introduced precisely for this purpose: to act as guarantees and to reveal the necessities which underlie and sustain the events which the inventory records. Sometimes, on the other hand, entities otherwise introduced are treated in some contexts as items in the inventory and in other contexts as guarantees. In either case, it was suggested, there is here interesting evidence of the complexity of the scientific tradition; evidence that science is not at all so whole-heartedly empirical, so uncompromisingly opposed to a-priorism, as is often supposed. It was argued that an interest in "making things intelligible" is in *conflict* with an interest in "making inventories of things", and that the distinction between describing and explaining is logically untenable. It is these latter points which I wish now to support: I shall argue that the scientist who accepts the assumption that explanation is concerned with revealing necessities has at once to accept and reject the view that *difference* is less real than, or less fundamental than, *identity*.[1]

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On that view of explanation which seeks guarantees for events, the scientific entities are not treated as things among other things; they are, rather, treated as the *sources* or *substrates of things*, and are thus given a special logical status. They are regarded as being essentially sources, and as not themselves having sources; ordinary things (the items in the inventory) are, by contrast, regarded as being essentially *outcomes* or *derivates* and as not themselves having, in the same sense, further outcomes. The logical dualism of such theories may, of course, show up in a variety of ways—it may be held, for example, that the special entities are not *locatable* in the same way as are the things they explain; that they do not, like ordinary things, have both a local habitation and a name. (The "smeared-out" electrons of some versions of quantum mechanics are a case in point.) It may, again, be held that the special entities are *active* and *effective* in ways in which the inventory items are not; or that they have a *permanence* and *persistence* not found among the ephemeral and mutable things which they sustain. Sometimes the essential *simplicity* and *homogeneity* of the special entities is contrasted sharply with the inescapable complexity and diversity of ordinary things. And commonly, in all these cases, the logical dichotomies go along with epistemological dichotomies; it will be held, for instance, that the special entities are known by *reason*, and ordinary things known by (or in) *experience*. But however the distinction is made it remains true (I shall contend) that logical dualism leads to a position in which theorists have at once to recognise and to discount *differences*.

When the special entities are treated as *sources*, things and events are made "intelligible" by being exhibited as *emergences*, as the *realisations* of potentialities, as the *unfoldings* of natures, as the *workings-out* of principles. On this view, the events must in the last resort be aspects of, or must somehow be contained in and be due to be evolved from, the entities which sustain them—only so can the desired necessity be got. This, it may be suggested, is often the central point (or at least the chief point of *philosophic* interest) of such explanatory principles as the "Principle of the Uniformity of Nature", and the principle that "*causa aequat effectum*"; this too is often the point of philosophic interest in theories of "evolution" and more generally in theories which seek explanations in terms of *origins*. It comes out in at least some accounts of the several *conservation* theories of physics. The chemical doctrines of affinity and valence lend themselves readily to this approach; but its place in chemical theory is best illustrated in the fact that the chemist says, on the one hand, that a compound is *different* from its elements, yet on the other insists on writing formulae which suggest that elements are, after all, *latent* in compounds. In all such theories there is an assumption that true explanation, or complete understanding, discloses identity—identity perhaps hidden, underlying, and known only by reason (known only by its "effects"), but identity none the less. There is, more or less explicitly made, a distinction between appearance and reality—diversity and change are assigned to the realm of appearance, identity and permanence to the real. On such views, if the apparently independent events that happen are really different from their guarantors, then no reason has been found why they *should* happen, and the events remain incomprehensible; remain a mystery. It would be conceivable that they *might not* happen; and the explanation would have failed to achieve its purpose.

Now Nunn maintains that only *some* scientific objects come to be treated as sources or substrates: he distinguishes (to put it in his terms) between those scientific objects such as electrons, which are often taken to *replace* things, and those others such as caloric and ether which, he holds, are merely *added to* things; in line with this view he suggests (as it is generally suggested) that the notion of caloric was finally dropped because the assumed materiality of heat could not be squared with the observations of Rumford—observations which suggested that heat could be produced in unlimited amounts by mechanical processes. As against Nunn's view it may be held that any scientific entity can be, and often is, regarded as something underlying and supporting events; and that when it is so regarded it does replace things and does not merely take its place among them. In the case of caloric, Rumford's observations raised a difficulty only for those calorists who had already departed from the original doctrine. The original caloric was not at all regarded as another substance: it was, on the contrary, taken to be a *principle.* This is well brought out in the controversies over Black's doctrine of the latency of caloric in changes of state. Caloric had been regarded as that which persists through thermal changes, as the hidden power or principle which underlies and is the source of changes of (what we now call) temperature; Black then suggested that while sometimes caloric displays a power to bring about changes in *temperature*, at other times it operates so as to cause changes of *state.* In the former case the caloric is said to be functioning so as to be "sensible", in the latter case it is functioning so as to be "latent". Some of Black's contemporaries objected to this doctrine as a "manifest contradiction", and so make it clear that for them *the whole nature* of caloric consisted in its power to cause—to "appear as"—changes of temperature. Notice that the critics were not objecting to the *latency* of "latent caloric" in Black's theory. They too held that even "sensible" heat was yet always latent in the sense that it could never be isolated: they too took for granted that caloric could, in any case, be known only through its "effects". What they objected to was Black's supposition that caloric might have *either* of two effects: it might cause either change of temperature or change of state. On Black's theory caloric had no single nature; it no longer provided a straightforward guarantee, and so no longer did the job that such entities are invoked to do. I am not, of course, suggesting that a recognition of the failure of caloric to provide a guarantee was, historically, a reason for the decline of the theory; I am suggesting that in the original theory caloric was not one substance among others—was not, in Nunn's terms, just *added to* the inventory of things. It may not have been held to replace any "things"; but it was, sometimes at least, taken to replace (to underwrite and to act as the principle or source of) the temperature changes which go on—it gathered up such changes into itself, and then in the explanations they emerged, guaranteed, from it.

The case of caloric in fact illustrates rather well the dualism of much scientific thought. It was sometimes treated as an essence, as a principle from which explanations might be spun out so as to make events intelligible; and this alone required that the events be regarded as *derived from* the principle: required that facts be subordinated to necessities. But then further caloric itself came to be regarded as at once an essence and a thing among others; it came to be regarded as a *material principle.* In so far as it was regarded as material (i.e. as localisable—since it was generally held to be imponderable) it *was* imperilled by Rumford's observations, and more seriously imperilled by later work on the refraction and especially on the *polarisation* of radiant heat. But throughout most of the first half of the nineteenth century—for several decades after Rumford's attack—it continued to play its part as a principle; and its failure to behave as a properly material entity was not taken seriously. That failure was taken seriously, and the material facts were respected, only when another principle, energy, was ready to take over the explanatory functions of caloric.

We may note a similar state of affairs with phlogiston, the immaterial principle of combustion. It is sometimes supposed that the seventeenth and eighteenth century chemists regarded phlogiston as one substance among others, as another item in their inventory; and held that this doctrine was laughed out of court when, in order to explain the increase in weight of metals during calcination, it became necessary to regard phlogiston as possessing not gravity but levity. This supposition rather misses the mark. It is true that from time to time attempts were made to identify phlogiston with particular substances—with sulphur, for instance, and later with (what we call) hydrogen. But many supporters of the theory opposed these attempts to treat phlogiston in a matter-of-fact way, and insisted that because it was the very principle of combustibility it could not be a mere combustible, a mere participant in combustion. For them it was nonsense to suppose that someday someone might isolate and take hold of some phlogiston, and equally nonsense to suppose that an unconfinable spirit might possess *either* gravity *or* levity. For these chemists the "augmentation of calces" was a problem, to be sure; but it was not a problem bearing directly on the doctrine of phlogiston. They groped towards a solution of the problem in terms of other substances—particles of air, for instance—which might be induced by the principle of inflammability to participate in calcinations; phlogiston itself lay untouched behind their gropings. For them, phlogiston persists through combustions; its whole nature consisted in its power to cause and animate and wear the form of combustions. For them, as I would put it, combustion doesn't just *happen to* some things. What happens (in so far as anything, on this sort of doctrine, happens) is just that phlogiston is phlogiston; it unfolds its nature, the various combustions are aspects of this unfolding and, so regarded, are intelligible. The phlogiston to which levity was attributed was a later, hybrid, entity—at once and impossibly an essence and a thing. It too had become a material principle.

As for the ether, let us turn again to Sir Oliver Lodge. Watson, commenting on the passage quoted earlier, notes that

> "Even the continuous medium will not do for us what Lodge thinks it will do. The medium's function is to connect one part of space with

> another, and this appears to be assured because the medium is continuous and any part of the medium touches parts adjacent to it. Clearly, however, adjacent parts are not in the same place, so there still remains the problem of connecting them . . ."[2]

But this is not a full statement of the case. We could say, "*Either* adjacent parts are not in the same place, when there still remains the 'problem' of 'connecting' them; *or* adjacent parts are not separate"—and this latter is just the alternative which Lodge finally chooses when he says

> ". . . the fundamental medium filling all space . . . *must* be ultimately incompressible; otherwise it would be composed of parts, and we should have to seek for something still more fundamental to fill up the interstices"[3]

and goes on

> "The ether being incompressible, and an electron being supposed composed simply and solely of ether, it follows that it cannot be either a condensation or a rarefaction of that material, but must be some singularity of structure . . ."

and finally

> ". . . the ether is a perfect *continuum*, an absolute plenum, and no rarefaction (of it) is possible. . . . Ether is 'all there', everywhere, without break or intermittence of any kind; while matter has gaps in it—it is here and there but not everywhere."

So Lodge, in the end, explains away transactions across separations by denying that there are separations; the problem of interactions between parts of the ether is "solved" by denying that the ether has parts—and then re-introducing those parts as "singularities of structure"; as knots and vortices and the like. It is clearly enough nonsensical to talk of a *structure* in an "absolute plenum" without parts; it is clear too that just such nonsense will come out in any theory which at once seeks necessity in events *and* is not content to say, with Parmenides, just that "It is". In Lodge's theory the ether is at once identified with and distinguished from the transactions it explains. Transactions between separated things do not, in their own right, happen: what happens is just that ether is ether—but, fortunately, it contains singularities; centres of crystallisation about which its nature can commence to unfold; pegs upon which the equations may be hung. The conviction (such as it is) carried by the theory thus rests not at all, as Watson suggests, on the fact that "any part of the medium touches parts adjacent to it", but on the fact that certain transactions are exhibited as emergences from their guarantor, as aspects or consequences of a principle. Despite Nunn's contention, the ether does not take its place in the world along with other things, not even provisionally. Immediately, it replaces and sustains actions at a distance; finally it swallows up everything—as Lodge puts it, electrons are composed simply and solely of ether, and Kelvin assures us that "ether is the one thing whose reality and substantiality we are sure of". It is worth noticing that the ether, like caloric and phlogiston, held a place in scientific theory *so that* it might provide explanations, *and* that it could provide explanations only so long as it was *not* taken as one thing among others. When it was also so taken—when it was regarded in the dual way as a *material* principle—then there arose all those difficult questions about the motions of other things through the ether-thing: those questions which, historically, led to the rejection of the ether by the early theory of relativity. So long as the ether was left standing behind facts, or enveloping facts, it was safe, and might be used to seem to make the facts safe. But when it was also asked to take its place among the facts, it was lost.

As it is with the ether, so it is with mechanical theories generally; at least, so it is with mechanical theories when they are held to be the vehicles of apodictic certainty. A theory setting out to find certainty can pretend to find that certainty only if it can pretend to find that differences are unreal. Just as Lodge tells us that electrons are composed solely of ether, and that ether is an absolute plenum and is everywhere, so Heaviside pushes on the views of Helmholtz and du Bois-Reymond to the conclusion that

> "There are only two things in the world—matter and energy; everything else is moonshine",[4]

and his dictum is widely applauded around the laboratories.

It is applauded until it is noticed that the reduction is not complete; that we are faced with the distressing need of being sure of two things. Then it is time for someone, Ostwald for instance, to insist that matter too is moonshine and that there is only one thing in the world—energy. "All the properties of bodies", Ostwald[5] declares, "are definable in terms of energy" *and* (he holds) a "body" *is no more than* its properties. "Matter", he says, "is a notion we need make no use of whatever." And so we come to The One; but it turns out that we are not, after all, satisfied. The scientist cannot rest content with that denial of differences to which his search for certainties has led him. Ostwald forthwith goes on to assert that there are "different forms" of energy (just as Lodge found "singularities" in the ether), and he holds that different forms of energy have different properties, despite his earlier contention that differences in properties are to be understood as differences in energy. The Mechanical Absolute clearly behaves like any other Absolute: it gathers things up into itself, dissolving and smoothing away their differences. It keeps them long enough to label them as its own aspects, to stamp them with the seal of the True Reality; then it returns their differences to things, unrolls itself, and lets them scamper away. They are now intelligible.

It appears, then, that the search for explanations, when this is held to be different from "making an inventory of things and events", leads the scientist into a dilemma in which he has at once to recognise and to deny differences. Now this conclusion may seem to be incompatible with the widely-held view that explanation is the very business of science, and that, indeed, an interest in explanation is what distinguishes the pursuits of science from those of mere idle curiosity. It is generally assumed that "curiosity" is a motive satisfied with superficial knowledge, and is to be contrasted with a desire to know the reasons for things, and that it is this latter desire which characterises the outlook of the scientist. Leaving aside a discussion of "motives" (themselves perhaps another species of "scientific entities"), I would contend that knowing the "reasons" for things *is* superficial knowledge, and that the rejection, sometimes at any rate, of explanations is just what distinguishes science from idle curiosity. To put the point rather roughly for a start, it is just the idly curious person who, having asked, say, "Why do birds fly?", is then satisfied to be told "Because it is the nature of birds to fly". The person who is merely curious accepts explanations because they put an end to *puzzlement*; the person who genuinely wants to know what goes on rejects them just because the invocation of ultimates puts an end to *enquiry*. When Helmholtz was envisaging that scheme of explanations through which the "vocation of theoretic natural science will be ended", he was in fact envisaging not the *completion* of enquiry but the *abandonment* of enquiry. The fact that Helmholtz continued, actively and very successfully, the business of enquiry, is just plain evidence that sometimes he forgot his interest in "the reduction of natural phenomena to simple forces".

The opposition between the traditions of enquiry and the search for ultimates comes out clearly in Cavendish's defence of the phlogiston theory against Lavoisier. No one would deny that Cavendish was a most able investigator, nor that he was more fully imbued than most men ever are with a passion for knowledge of the world. But when he reviewed—fairly and lucidly enough—Lavoisier's account of the composition of water, he went on to comment:

> "It seems . . . as if the phenomena of nature might be explained very well on this principle without the help of phlogiston; and, indeed, as adding dephlogisticated air to a body comes to the same thing as depriving it of its phlogiston, and adding water to it, and as there are no bodies entirely destitute of water, and as I know no way in which phlogiston can be transferred from one body to another, without leaving it uncertain whether water is at the same time transferred, it will be very difficult to determine by experiment which of these opinions is the truest; but as the commonly received principle of phlogiston explains all phenomena, at least as well as Mr. Lavoisier's, I have adhered to that".[6]

It is worth noticing, incidentally, that water, the vehicle through which phlogiston acts—the point of attachment of phlogiston to the world—is held (like Lodge's ether) to be *everywhere*: we noted earlier that logical dualism sometimes comes out in the ascription to the special entities of unique *spatial habits*. But what is most striking here is the discovery of Cavendish, a very prince among precise experimenters, *avoiding* a crucial experiment; declining, on grounds of uncertainty and difficulty, to submit his principle to the trials of observation. It would, no doubt, be grossly unfair to compare Cavendish with those colleagues of Galileo who are said to have refused to look through the telescope, lest they see for themselves the imperfections and evidences of mutability in the sun and the moon. But the parallel between phlogiston and Aristotle's abiding quintessence is clear enough; and in each case the principle had to be protected from contamination by mere facts; the explanation had to be regarded as the *last* word.

It is important to note that Cavendish misses entirely the point of Lavoisier's criticism. He *defends* phlogiston as a "principle (which) explains all phenomena"; but this is precisely Lavoisier's *complaint* against it:

> ". . . les chimistes ont fait du phlogistique un principe vague qui n'est point rigoureusement défini, et qui, par conséquent, s'adapte à toutes les explications dans lesquelles on veut le faire entrer".[7]

Phlogiston is a slippery Proteus—and is therefore to be preferred, by those who seek explanations, to the limited and intractable oxygen. The point here is not that Lavoisier was right, where Cavendish was wrong, about some facts. It is not a straightforward choice between a substance, phlogiston, which is *disengaged* from burning substances, and a substance, oxygen, which *enters into* burning substances. The choice primarily is between a principle which explains combustions, and a substance which participates in combustions—which has, literally, a place in them. Cavendish's choice of the principle which explains is attended by a disregard of what goes on: it is "very difficult to determine". To the question, "Why does oxygen cause burning?", Lavoisier has, of course, no answer; but the question, properly taken, opens up a new field of enquiry. To the question, "Why does phlogiston cause burning?", Cavendish has a *complete* answer: "Because that is the very nature of phlogiston!"—and the answer prevents further questions.

Lavoisier's theory carried the day, and combustion became a matter of mere fact. But there was still much for the essences to do in this field: Lavoisier himself, for instance, sometimes regarded oxygen as the *acidifying principle*: as that to which the properties of acids are "wholly due". Again, he still rather doubted the substantiality of *gases*. For the father of modern chemistry, gases were still just a little ghostly; pneumatic chemistry was still dealing with spirits. The *matter of oxygen* he could comfortably envisage as solid, real, and earthy; but another principle was needed to assure that the matter of oxygen could adopt the ghostly state without becoming a mere ghost. That other principle was, again, caloric—"latent" caloric now; and Lavoisier held that what participates in combustions is the matter of oxygen sustained in the gaseous state by the principle of heat. Dalton at one stage of his work held a similar view, and we shall see that it played a considerable part in leading him to reject the results of Gay-Lussac's important experiments on the combining volumes of gases. That rejection, in turn, made it impossible for him to put the atomic theory on a sound basis, and went far towards undermining the confidence of his contemporaries in his whole system. But before I take this up I must stress that I am not seeking merely to draw up a list of The Outstanding Errors of Science. The contention to be illustrated is simply that an interest in necessities and a pre-occupation with explanations lead to a minimising of the significance of observations. We shall see in this case that Dalton sets up two sorts of necessities and is led on two counts to refuse to take seriously the important facts which Gay-Lussac put squarely before him.

Dalton first sought to explain the fact that the gases in the atmosphere do not separate into distinct layers; and, like

Kelvin after him, took for granted that an explanation here would consist in setting up a mechanical model: a model whose behaviour could be laid down with certainty. He had taken over from Newton the assumption that "God in the beginning formed matter in solid, massy, hard, impenetrable, movable particles", and from Lavoisier the assumption that the solid particles are, in the gaseous state, enveloped by "an atmosphere of heat". He held too that heat possesses an innate repulsive force—a force revealed in the expansion which substances undergo when they are heated—and in this way the permanence of the gases was guaranteed. Atoms could not coalesce because the envelopes of heat about them pressed on and forced back their neighbours. The problem then was to demonstrate that the gases of different species in the atmosphere must mingle with one another. This was achieved very simply by supposing that the atoms of any one gas (each taken along with its envelope of heat) were different in *size* from the atoms of any other gas. If the atoms of different species were supposed the same in size, then (Dalton argued) the mutual repulsions would soon bind them down to rigid and unchanging arrays; but if their sizes were different then their repulsions could never balance, and the atoms of one size would be *obliged* to slip between and mingle with those of another size—diffusion and the absence of distinct layers were assured.

Dalton's arguments are supported by diagrams in which lines of force emanating from the atoms are shown meeting and balancing when the atoms are of equal size; failing to meet and failing to balance when the atoms are not of equal sizes. They are, throughout, just such arguments as are generally held to carry apodictic certainty. Now a central tenet of the assurance thus achieved—a clause in the policy—is that the atoms of different species shall be of different sizes; and it is just this clause which, as Dalton saw it, would be invalidated if Gay-Lussac's results were allowed to stand. Gay-Lussac's observations implied, for Dalton, that equal volumes of the elementary gases contained equal numbers of atoms; this in turn implied for him that the atoms of the several elementary gases must be of the same size. And that could not be allowed: it would render null and void his policy insuring the commingling of the gases in the atmosphere. So Gay-Lussac's work was rejected, even made fun of. Sir Henry Roscoe,[8] commenting on this, seeks to excuse Dalton on the ground that "Dalton, it must be admitted, was not great in experimental chemistry", and quotes from Angus Smith the remark that "Nature . . . had destined Dalton exclusively for the lofty rank of a law-giver in chemical science". But what comes out here is the fact that "the lofty rank of a law-giver" is, when explanations are at stake, apt to become the rank of the *censor*. Berzelius realised that Dalton's procedure was indefensible; he saw that Gay-Lussac's results could be taken so as to strengthen the empirical bases of the atomic theory. He appealed to Dalton to admit the facts and to revise the theory —to revise, at least,

> "Cette partie . . . qui vous nécessite de déclarer les expériences de Gay-Lussac sur les volumes des gases qui se combinent pour inexactes. J'aurais cru plutôt que ces expériences étaient la plus belle preuve de la probabilité de la théorie atomistique, et je vous avoue d'ailleurs que je ne croirai pas si aisément Gay-Lussac en defaut, surtout dans une matière où il ne s'agit que de mésurer bien ou mal".[8]

But, as Roscoe says, Dalton remained obdurate.

Now Dalton had a further reason for obduracy in the face of the facts: another insurance policy to be maintained. Newton's massy particles, taken up in order to account for the physical properties of gases, had turned out to be useful in explaining certain of their chemical properties—especially the laws of constant and multiple proportions. An important clause in *this* policy, then, was the condition that the particles should be, by nature, indivisible: that they should be *atomic*. As *atoms* they guaranteed the quantitative laws. Dalton, in a letter to Berzelius, puts it like this:

> "The doctrine of definite proportions appears to me *mysterious* unless we adopt the atomic hypothesis. It appears like the *mystical ratios* of Kepler, which Newton so happily elucidated".[9]

We have seen that other scientists have regarded Kepler's laws as mysteries needing elucidation; .we have seen that the attitude of regarding brute facts as mysteries goes along with the seeking out of "causes which ordain". For Dalton, then, the *atomicity* of the particles, like their *variety of size*, was a cause which ordained; and was as such to be maintained inviolate against any evidence to the contrary taken from the world of mere contingency. Here therefore there were for Dalton further grounds for refusing to take seriously the work of Gay-Lussac. Gay-Lussac had claimed to have shown that there were simple relations not only between the volumes of the gaseous elements entering into chemical combination, but also between these and the volumes of the *compound* gases so formed: in some cases at least his results implied that the compound atoms came to be formed from *half-atoms* of the elements. Dalton's answer was at least succinct: "Thou knowest thou canst not cut an atom".

The immediate result was bad enough—Dalton refusing, on grounds which many of his contemporaries saw to be unsound, to admit plain facts; more remotely the consequence was that there remained closed to him the one path which could lead him from equivalent weights to unambiguous determinations of relative atomic weights and molecular formulae. That path was built, on just such facts as Gay-Lussac was bringing forward, by Avogadro, Ampère, and Dumas; but Dalton could not follow it, and was compelled to fall back, for the numerics of his system, upon considerations of simplicity and of harmonious patterns of atoms. The confusions raised by Dalton's rejection of Gay-Lussac's work, and his acceptance of intuition as the only available guide to molecular formulae, were largely responsible for the long and tragic neglect of Avogadro's brilliantly illuminating work; responsible largely too for a growing scepticism and a move towards the abandoning of theory altogether. Wollaston, as early as 1814, sought a return to the use of equivalents only:

> "I have not been desirous of warping my numbers according to an atomic theory, but have endeavoured to make practical convenience my sole guide";[10]

and Dumas spoke for many when he said

> "Si j'en étais le maître j'effacerais le mot *atome* de la science, persuadé qu'il va plus loin que l'expérience; et jamais en Chemie nous ne devons aller plus loin que l'expérience".[11]

The historian Pattison Muir[11] chides Dumas for this, suggesting that Dumas is "forgetting that the atomic theory is merely a mode of expressing the results of experience". But Dumas is forgetting nothing of the sort; it is Pattison Muir who misunderstands the status of the theory. For Dalton "the results of experience" were mysterious; the atomic theory had not to "express" them but to *explain* them; to this end it had indeed to go beyond experience and to discount it; the outcome, we said, was a movement for the abandoning of theory. The case of Dumas is particularly interesting: he struggled valiantly against the metaphysics of Dalton (and, we shall see, of Berzelius) and then, at the age of 48 and at the height of his powers as critic and investigator, withdrew from chemistry to take up political work. We would, no doubt, be taking violent liberties with history if we held that Dumas's withdrawal from active chemical research was a consequence of his disgust with the prevailing temper of chemistry; but it is a fact that he did for a long time play a leading part in the struggle against a-priorism; a fact too that the campaign was especially *disheartening* just because "causes which ordain" are Protean, shifting, adjustable, non-locatable entities. They won't stay to be examined. So even if we cannot aver that Dumas retired from chemistry in disgust and despair, we can see that it would not have been surprising if he had; we can say with conviction that at this time the spirit of enquiry was, within chemistry, sorely oppressed by the dogmas of the seekers after explanations.

The supreme "law-giver in chemical science" at this time (about 1820 to 1845) was Berzelius; his position in the contest between a-priorism and empiricism is especially important.

With him, as with others we have noted, the struggle is within his own mind as well as beyond it. We saw his appeal to Dalton on behalf of Gay-Lussac; on another occasion he took Dalton to task on the ground that

> ". . . the small number of analyses given show (Dalton's) desire for certain results; but this is the very attitude we should avoid when seeking proofs for or against a theory".[12]

Yet this same "attitude we should avoid" begins to show up when he agrees with Dalton that the empirical quantitative laws are indeed mysteries needing to be made intelligible; it comes out nakedly when he rejects some important experimental results obtained by Dumas and others—results of experiments on the substitution of chlorine into organic compounds—on the ground that

> "An element so eminently electronegative as chlorine can never enter an organic compound—this idea is contrary to the first principles of chemistry".[13]

Now I am not trying to suggest that there is anything unscientific in having theories, nor that "experimental results" are to be set above properly-understood "principles": on the contrary I am trying to illustrate the contentions that there are no degrees of truth, and that a coherent account of scientific theory demands a recognition that any proposition is at once specific and general: at once an "observation" and a "principle". It is, roughly, functioning as an observation when we simply take it to be true; it is functioning as a principle when we are using it as a premise in deductions. We do not have on the one hand "observations" having one kind of truth, and on the other "principles" having another kind of truth: we have simply propositions which can function, in our investigations, in different ways. Berzelius, we shall see, *is* treating his "first principles" as having a kind of truth superior to that possessed by observations; is treating them as entities to be protected against the facts.

The electric polarities first entered Berzelius's work in his accounts of the electrolyses of inorganic salts; their inflation to higher-order entities comes out when he lays it down that

> "What we call chemical affinity is nothing but the action of the electric polarities of the particles, and electricity is the first cause of all chemical activity. . . . every chemical action is at bottom an electrical phenomenon, founded on the electric polarities of the particles".[14]

This, you see, is not just fact; this cannot be taken as observed. This is metaphysics—"nothing but . . .", "the first cause of all", "at bottom"—and we are on the way towards explaining things away. No doubt if we asked how *this* case of "affinity" differs from *that*, when each is held to be "nothing but" the action of electric polarities, we should be told that the underlying actions differ in magnitude or intensity or in arrangement—we should, in effect, be referred back to Pythagoras—but the point is that qualitative differences are to be set aside. On this theory chemical substances are seen as outcomes or derivates, and they have, finally, no properties of their own. What they are is, at bottom, wholly due to the first cause.

In Berzelius's theories of organic chemistry (which alone I shall consider) there are in fact *two* sorts of "first cause", two sorts of entities to be protected, two grounds for distinctions between what appears to be and what really is. There are, to begin with, "radicals"—entities compounded of carbon and hydrogen and held to be, in organic transactions, uniquely *persistent*, uniquely *effective*, and rather mysteriously *located*. The radicals belong to reality and not to appearance; they play much the same part here as do "elements" in the explanatory, necessity-seeking, accounts of inorganic chemistry. The radicals pass unchanged through transactions; compounds are mere transient stages in the re-arrangings of the abiding realities. The radicals have their own assured properties; the compounds have only such properties as result from or are lent them by the underlying radicals. The radicals do not show themselves in compounds: they are substrates, known by their effects, known by reason. Now we have seen that theories of substrates are unstable: that the explanatory principles so set up tend to be regarded ambivalently as *material principles*—and this is precisely what happened here. On the one hand many attempts were made to *isolate* radicals; but what were thus isolated were of course just further substances; substances transparently mutable, limited, and only mundanely real. One could not seriously hold that these guaranteed anything else; just as at an earlier time it could not seriously be supposed that common or garden "flowers of sulphur" were the very principle of inflammability. And so on the other hand the search was pursued at the writing desk rather than at the laboratory bench. Some historians describe the ensuing battle of the paper bullets with an air almost of bewilderment, as though this were an incredible aberration from the "scientific method"; a folly best forgotten. The point is, of course, that if explanations are hunted, only paper bullets will bring them down: they are not to be ensnared in flasks or retorts. In any case, the substrate radicals were produced in great profusion: and, free from the limits of the mundane, they served to explain—but the facts suffered grievously. Only those reactions were sought, or being found were taken seriously, which seemed to reveal the "effects" of the radicals; atomic and molecular weights were revised, almost daily, just so that formulae could be written which would portray the radicals hiding in the compounds. Berzelius for some time opposed the growing retreat from the facts—

> "I must insist that formulae should be received only when the ideas which they seek to express have some claim to be acknowledged as proved facts, otherwise they can but lead to confusion like that of Babel";[15]

but, with first principles at stake, his opposition wavered and collapsed: he came to play a leading part in the pursuit of these gilt-edged wills o' the wisp—a pursuit which did much to bring organic chemistry to a state of confusion, eclecticism, and finally to a widespread sceptical distrust of all theory.

But this was not all. Berzelius sought further to guarantee the combinations of the radicals; and here his other "first cause" comes in. The radicals are held to be electrically polarised; they combine with oxygen, the most electronegative of the elements; these combinations sometimes possess residual polarities, and they may then further combine. Thus all combination is assured; all is held together by the uniquely pervasive and omnipresent force of electricity. At this stage of the theory the radicals themselves lose their sanctity and power; everything that happens is seen, finally, as a manifestation of the activities and powers of the one cause, electricity.

Dumas saw clearly that this was a metaphysical inflation of a notion which has a perfectly valid meaning in the limited, factual, context of electrolysis:

> ". . . the role of electricity can be observed only at the moment when the combinations are formed, or at the moment when they break up. But, when the elementary molecules have attained equilibrium, we no longer know how to define the influence their electric properties are able to exert, and no one has yet expressed views on this subject which are in accord with experience".[16]

Dumas brought forward cases in which the "eminently electronegative" chlorine replaces the much less eminently electronegative hydrogen *without* any marked change in the properties of the compound. Such cases impugned the status of Berzelius's first cause: they implied that the properties of compounds are *not* "wholly due to" the electric states of their elements. And so here, with a principle again at stake, Berzelius allowed *his* "lofty rank of law-giver in chemical science" to become that of censor; he used his censor's scissors to perform some remarkable dichotomies on Dumas's seditious compounds. He cut these compounds into two parts—one, the *essentially* organic part, the other (called the copula) a sort of rump or addendum or epiphenomenon. The essentially organic part was taken as guaranteeing the properties of the whole compound; and within *it* the electrochemical laws reigned supreme. It had to consist of a carbon-hydrogen radical joined to oxygen; other elements were debarred from entry into it. So long as it remained inviolate the properties of the whole compound remained substantially unchanged; it was, in short, the soul of the compound. The copula, on the other hand, did not in the same way determine properties, and was itself electrochemically undetermined. Other elements could come into it and go from it as they pleased; it could link troublesome, electrochemically recalcitrant, elements like chlorine to the organic essences without any serious alteration in properties. So the first cause was saved; but the accepted facts of analysis received a rough handling. In order to perform his operations, in order to find an acceptable soul *and* a copula, Berzelius had often to fatten his patients first: he had to double, or otherwise alter, many accepted values for molecular weights—and to alter them for no other reason than this. More than that, he was led to cast doubts on the value of the methods then being elaborated for the unambiguous determination of molecular weights. He *needed* the flexibility of his "chemical analogies"; he had to *avoid* the unequivocality of Avogadro's methods. He had, as we saw, once warned chemists that a-priori tamperings with formulae would lead them to a "confusion like that of Babel"; but with Dumas's facts to be explained away he was prepared to justify downright mysticism:

> "When I see an incorrect theoretical representation I feel, even when the correct one is unknown to me and when I cannot make it clear to myself, that it is wrong, just as when the ear apprehends a false note in music: it is something of the same kind as when one recognises bad form by feeling rather than by reason".[17]

Such was the obscurantism against which Dumas struggled until he turned to politics: it is indeed no wonder that by 1840 many chemists were abandoning the atomic theory and were returning to the use of equivalents—quantities having no directly spatial significance at all. The return to equivalents was serious enough in inorganic chemistry; in organic chemistry it was calamitous. The phenomena of isomerism, cropping up throughout organic chemistry, are entirely intractable on the basis of equivalents alone. The instability and confusions of organic chemistry at this time are such that some modern histories of chemistry give up the attempt to portray them—it was, almost, a war of all against all.

If then we say that Berzelius thoroughly deserved Dumas's rebuke:

> "These electrochemical conceptions—this special polarity which has been assigned to the elementary atoms—do they really rest on such evident facts that they must be accepted as articles of faith?";[18]

if we say that Berzelius, in his role of defender of that faith, was playing the part of a chemical Procrustes and mutilating the body of accepted facts, we are not at all prevented from recognising the value of his many contributions to chemical science. We might, indeed, regard him as a tragic figure, for *he* had largely built up the very structure which his own search for certainty led him to undermine.

I have sought to illustrate the contention that the pursuit of guarantees profoundly affects the shape of scientific thought. But it may now be suggested that I have been flogging a dead horse: that I have overlooked the growth of that "critical movement" in science for which Karl Pearson claimed to speak when he announced that

> "Nobody now believes that science explains anything; we all look upon it as a shorthand description; as an economy of thought".[19]

It may be pointed out that when Ernst Mach rejected Newton's definition of mass as a "quantity of matter" and substituted for it a definition of mass as a ratio of accelerations, Mach then said that his definition was

> ". . . the outcome of an endeavour to establish the interconnectedness of phenomena, and to remove all metaphysical obscurities";[20]

and that necessary connections (as distinct from interconnections of phenomena) have come to be so widely regarded as "metaphysical obscurities" that most scientists would agree as a matter of course with Professor Dingle when he writes

> ". . . to say that we have 'explained' a phenomenon is to suggest a kind of finality which is unattainable by science. Every scientific 'explanation' will be found on examination to be the establishment of a relation between the phenomenon 'explained' and other phenomena".[21]

The theories of relativity and of quantum mechanics may be cited in support of Pearson's view: it may be pointed out that each of these is now generally held to begin from the rejection of "unobservables", to insist on the primacy of observation, and to abandon attempts at explanation.

So it may seem that this paper has been no more than an exercise in antiquarian fossicking: that the scientific entities I have held to be the vehicles of an unscientific pursuit of certainty are now seen by all to be mere chimeras, unworthy of the scientist's attention. On this there are, I think, two things to be said. In the first place, it has not at all been my main purpose to *refute* the view that it is the business of science to explain things and to discover necessary connections. I do take it that that view is unsound; but if the argument of this paper has been correct, there is little point in seeking, once and for all, to refute that view. Pearson's statement that "Nobody now believes that science explains anything" is, as it appears in the oddly complacent Preface to the third edition of *The Grammar of Science*, not far short of a silly boast; and it is, in any case, false. People, including scientists busy at their own work, do go on seeking necessities, even when sometimes they themselves recognise that necessities are not to be found—Pearson himself, as we shall briefly see, is a case in point. There is nothing in the least paradoxical in the suggestion that a man may sometimes disown explanations and yet sometimes seek them; indeed, the history of scientific thought provides any number of examples of just this ambivalence. Ostwald, whom we found asserting that only energy exists, is elsewhere at pains to make it clear that scientific laws do *not* state necessary connections. Just so, Helmholtz introduces that discussion in which he concludes that the vocation of theoretic natural science will be ended as soon as the reduction of natural phenomena to simple forces is complete, with the assurance that what he is about to say is "purely . . . a physical premise, independent of metaphysical considerations". The editors who rejected his paper thought otherwise; and they were right. Helmholtz was trying to be at once a metaphysician and an empiricist. A good deal of light is, I think, thrown on the growth of science if we recognise the scientist as often embroiled in just such a conflict of opposed outlooks: indeed it is tempting to suggest that while an interest in knowing what goes on may be necessary for sustained enquiry, perhaps that interest alone is not sufficient. Perhaps some interest in "explanations", in ultimates, is also always one of the motives of sustained and enthusiastic enquiry: perhaps always the scientist is kept at his work partly by the conviction that he is approaching a final and complete solution to his "problems"—*even though* this conviction conflicts with his interest in facts. This may seem to be a rather scandalous suggestion; but I think it is one to be taken seriously.

But further I would argue that the rejection of metaphysical obscurities by Mach, Pearson, and their followers is neither soundly-conceived nor thorough-going. When for instance we find that Mach declares that his definition of mass as a ratio of accelerations rests on the alleged fact that

> ". . . mechanical experiences clearly and indubitably point to the existence in bodies of a special and distinct property determinative of accelerations"[22]

we may well wonder whether his criticisms of Newton do go to the heart of the matter. "A special and distinct property determinative of accelerations" looks very like another power, another virtue: we seem to be back among the metaphysical obscurities. When Pearson mentions the "very important possibility"

> ". . . that not only special modes of motion, but much of the mechanism which describes the motion of sensible bodies, will be found ultimately to be involved in some wide-reaching conception of ether and atom",[23]

and goes on to declare that

> "It is not logically satisfactory to describe one mechanism by another of equal complexity; and we must hope to ultimately conceptualise an ether from the simple structure of which the several laws of motion postulated for particles of gross 'matter' may directly flow",

to lay it down that

> "Either ether or matter must be placed in the more fundamental position, and there is no doubt now that the priority must be given to the ether . . .",[24]

he makes it clear enough that he still wants necessities, and that we may have them *so long as we are careful to call them "conceptual"*.

I shall not attempt any extended discussion of the "critical movement" in science—the movement generally held to begin with the publication of Mach's *The Science of Mechanics*; the movement whose views are roughly summarised in the contentions that there are specially scientific entities, and these are fictions of the mind; that there are specially scientific propositions, and these are shorthand conceptual schemes designed to promote the economy of thought. I would, of course, concede that the members of this movement have carried out some skilful surgery on the body of scientific theory with the help of Occam's famous instrument; but I would suggest that they misunderstand the cankers they remove. They reject final causes and ultimate laws and explanatory principles because these are, as Mach puts it, metaphysical obscurities. They fail to see the precise illogicalities in the rationalist doctrines they criticise, and they allow the same illogicalities to stand in their own doctrines. They will not allow matter or energy or ether or atoms or electrons to be more than ordinarily real —but they require them to be less than ordinarily real. They save the world from the essences by calling the essences concepts: they escape the conclusion that electrons are the very stuff of the world by adopting the view that electrons are mere inventions. They do not see that what are invented are precisely the essences; they do not see that their own doctrine of mental fictions reinstates the essences.

The point is that the critics, anxious to show that science is under no obligations to produce necessary connections in order to make things intelligible, declare that there are no connections in things at all; they hold that things are to be made intelligible by the setting up of conceptual connections and by the fabrication of conceptual scientific entities. The critics agree with the criticised that things, as they are, are not intelligible; and while the latter are led thereby to deny the reality of differences, the former begin by denying the reality of connections—and then they re-introduce those connections as "conceptual schemes": as propositions which are neither true nor false but are, at best, more or less convenient. As Stallo puts it

> "The difference between phenomena is a primary datum of sensation . . . thought proper begins with the apprehension of identity amid phenomenal difference. . . . Objects are *perceived* as different; they are *conceived* as identical by an attention of the mind . . . Thought deals, not with things as they are, but with our mental representations of them. . . . The sciences are conversant about single relations or groups of relations established (and, within the limits of the constitutive laws of the mind, *arbitrarily* established) by the mind itself . . ."[25]

It is just this point which Nunn takes up against Mach, when he maintains that some connections between "sensa" are "inevitable"; when he denies, on behalf of the "new realism", Mach's contention that

> ". . . the world is composed not of 'things' as its elements, but of . . . individual sensations",[26]

and opposes Mach's conclusion that

> "Only in the mind, therefore, does the mutual dependence of certain features exist".[27]

Against Mach's sensory atomism Nunn maintains that some of what he calls the "forms of association" of sensa are just as "objective" as the sensa themselves. As we saw, this doctrine of Nunn's amounts to an abandoning of the "sensa" theory. It needs, for its proper statement, an explicit recognition that we are directly confronted with complex states of affairs—states of affairs in which both differences and connection are already found, and for which there is just no question of rendering intelligible the initially unintelligible. If this is recognised it becomes clear that Mach and Pearson and Stallo, when they suppose that the only alternatives before us are fundamental, ultimate, unobservable entities on the one hand, and fictional, conceptual entities on the other, are supposing that if there were real connections, they would be identities; and that if there are differences then there are no real connections.

In this paper we have considered chiefly some approaches to scientific theory which assume, finally, that real connections are identities. We have seen that such theories, in order that they may have something to say, must become dualistic: must, in one of a number of ways, distinguish appearance from reality, must distinguish essences from aspects. We have seen the illogicality of such theories illustrated in the fact that those who hold them have at once to recognise and to deny differences; we have seen how the frame of mind which goes with such theories is at odds with the generally empiricist traditions of science. It may be suggested that a similar dualism, a similar illogicality, and a similar departure from the central scientific tradition is to be found in those other, and now more fashionable, theories which assume, finally, that differences are incompatible with real connections. Against both it may be maintained that difference and connection *go together* in states of affairs; that there are no logically unique scientific entities or scientific propositions; and, incidentally, that there is no unique philosophy of science.

It remains to say a word about Dr. Brown's "metaphysical hoax" from which we began. It is rather a pity that modern English has departed so far from the original meaning of "invent" that we now use the misleading "discover" to mean *come upon*. But taking the two words in the senses in which we now use them, it is perfectly clear that Sir J. J. Thomson is entitled to the claim that he *discovered* the things which coursed through his evacuated tubes, were deflected by his electric and magnetic fields, and accumulated on his electrometers; clear enough that Thomson's electrons take their place in the scientist's inventory of things and events. There is nothing the least bit philosophically alarming, and no suggestion of any hoax, in Dr. Brown's fact that what Thomson discovered "behaves in some circumstances more like a wave disturbance than a particle". No one claims to know all about what Thomson discovered. It is equally clear that those pseudo-entities, also called electrons, which sometimes are held (along with protons, neutrons, neutrinos, *et hoc genus omne*) to be the underlying bases of things, to be the very font of all activity, to be the abiding reality behind the insubstantial flux of things—these indeed are inventions and fictions; to claim *them* as discoveries would indeed be to attempt a metaphysical hoax.

Notes

[1] Mr. C. F. Presley has pointed out that the examples and arguments of this paper are, in several places, quite similar to those of Emil Meyerson in his *Identity and Reality*. I have not yet looked at Meyerson; but I understand that his conclusions are rather different from mine.

[2] W. H. Watson: *On Understanding Physics*, 1938, p. 69.

[3] Sir Oliver Lodge: *The Ether of Space*, 1909, pp. 82 ff.

[4] Quoted by T. P. Nunn: *Animism and the Doctrine of Energy*, Proc. Aris. Soc., N.S. Vol. XII, p. 53.

[5] W. Ostwald (trans. Morse): *The Fundamental Principles of Chemistry*, 1909, p. 6.

[6] G. Wilson: *The Life of the Hon. Henry Cavendish*, 1851, p. 252.

[7] Quoted from J. H. White: *The History of the Phlogiston Theory*, 1932, p. 152.

[8] Sir H. E. Roscoe: *John Dalton and the Rise of Modern Chemistry*, 1895, p. 158.

[9] Ida Freund: *The Study of Chemical Composition*, 1904, p. 300.

[10] Ibid, p. 339.

[11] M. M. Pattison Muir: *A History of Chemical Theories and Laws*, 1909, p. 130.

[12] Ida Freund: *The Experimental Bases of Chemistry*, 1920, p. 270.

[13] J. R. Partington: *A Short History of Chemistry*, 1937, p. 242.

[14] Pattison Muir, op. cit., p. 248.

[15] Ibid., p. 243.

[16] Ibid., p. 257.

[17] Partington, op. cit., p. 244.

[18] Sir E. Thorpe: *Essays in Historical Chemistry*, 1911, p. 339.

[19] Karl Pearson: *The Grammar of Science*, Ed. III, 1911, p.v.

[20] Ernst Mach (trans. McCormack): *The Science of Mechanics*, 5th Eng. Edit., 1942, p. 267.

[21] Herbert Dingle: *Science and Human Experience*, 1931, p. 16.

[22] Mach., op. cit., p. 266.

[23] Pearson, op. cit., p. 348.

[24] Ibid., p. 370.

[25] J. B. Stallo: *The Concepts of Modern Physics*, 2nd Edit., 1882, p. 130.

[26] Mach, op. cit., p. 580.

[27] Ernst Mach (trans. McCormack): *Popular Scientific Lectures*, 4th Edit. (no date), p. 199.

THE FREUDIAN REVOLUTION*

By John Anderson

Freudianism, like Darwinism, may be called revolutionary in that it not merely introduces fresh conceptions into a particular field of inquiry but in some measure affects all our thinking, bringing about a general revaluation of ideas. It is true, of course, that, after the first shock, such influences are hard to sustain and readily collapse into vulgarisations; this is illustrated in the extent to which the catch-phrase "wishful thinking" has replaced study and understanding of the Freudian doctrine of the wish, and vulgar notions of "the struggle for existence" provide a parallel case. But one important feature of Freudianism is that it explains this very fact, that it exhibits the protection of customary views by a pretended assimilation of new ones as conforming to a regular mechanism of defence.

Vulgar notions of Freudianism as a general sexualising of human activities or as a clarion call against "repression" have some countenance in the work of Freud himself—in the doctrine of the "libido" as the single source or reservoir of mental energies and in the treatment of the repressed as the real person, the repressing factors being imposed or "introjected". But there is much in Freud's important, early works that would caution us against such "wild" interpretations; there is a constant insistence on conflict and on the fact that the conflicting tendencies are alike parts of the person, so that solution is not to be found by mere excision.

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* Based on an address given in the University of Sydney on April 8, 1952.

It is characteristic of a revolution to be a revaluation—to be a revolution in ideas, not a mere alteration of externals or change of fashion. Fashionable trends in psychology (the interest in gadgets, questionnaires, etc.) betray, as contrasted with Freudianism, a fear of fundamentals. The opposition is between systematic thinking and miscellaneous inquiries; a revolution, as against a relapse, in ideas involves the breaking down of divisions, the discovery of unsuspected connections, the establishment of continuity against a postulated discontinuity. And, in its working out of an objective view of mental life, in its attack on this stronghold of subjectivism or "separatism", the Freudian revolution, like all other revolutions, has an essentially philosophical character. The continuity of the mental and the bodily, of thought and action, the breaking down of divisions between conscious and unconscious, between normal and abnormal—these are Freudian contributions to thought in general, and they illustrate the revolutionary character which Freudianism has in common with Darwinism in their rejection of what has been the greatest obstacle to vigorous and systematic inquiry, the dualism of Man and Nature.

The general revolutionary character of Freudianism, then, lies in its naturalism, in its being a contribution to the natural history of man and mind. But what is important in the specification of that character is not sexuality (in spite of the immense amount of interesting material produced by Freud and his school on the varieties and disguises of sexual impulse) but conationalism as opposed to cognitionalism, the treatment of mind as a set of drives or urges and not as an abstract cogniser, the possessor of little bits of cognised content called "ideas".

Though Freud has played a considerable part in the replacing of associationism, the doctrine of individual ideas with their attractions and repulsions, by a doctrine of interests, of activities which attend to or turn away from various objects, he still finds an associative method, the seeking of the "mental associates" of any given content, to be the most profitable way of investigating mental processes. It may be said, indeed, that Freud's emancipation from cognitionalism, from the associationist doctrines which prevailed in the period of his early studies, was incomplete. But the point is that we take a very different view of the search for associated ideas (for "latent content", etc.) if we regard the association as the work of active interests from that which we take if we think the association is the work of ideas themselves—and, with interests as our guiding principle, we are likely to *find* much relevant material that we should otherwise overlook. The notion of analysis would be pointless without the connected notion of the synthetic force of mental activity.

It is in terms of activity that the notion of disguise or substitution becomes intelligible; we are to think of a mental force as seeking outlet, encountering a barrier on its path, spreading sideways along associative lines, and then finding a way out in the general direction of its original pressure. And that these conceptions of flowing, damming, etc., are not merely figurative, that there is nothing wrong with a *physical* description of mental processes, is shown or at least corroborated by Freud's account of the bodily symptom—of its functioning, similarly to the "intellectual" response, as part of the language of the emotions. Nevertheless the classical form of analysis, the investigation of mental forces or drives in terms of their objects or cognised materials, has proved particularly fruitful and is not misleading so long as it is understood that the forces and what they are directed towards are not the same thing. It is certainly misleading to speak of *the* analysis or *the* interpretation of a dream or other mental phenomenon involving substitution and disguise; it has to be understood that there is no complete formula for any mental (and equally for any non-mental) situation, that, whatever of its connections are discovered in a particular investigation, others exist as well, and thus that it can have various "interpretations". But this is nothing against the exactness of any given set of discoveries or mass of exposed material; and it is an outstanding merit of the Freudian procedure that it insists on the definiteness of connections, on mental mechanism or determinism in terms of which alone there can be study of human character or a human character to study.

It is with reference to dreams that the various mechanisms of distortion and substitution have been most clearly and fully presented; and while the formula for the dream, "the realisation of a suppressed wish", has been shown to apply to such varied phenomena as forgetting, error, phantasy in general and even symptom-formation, it may still be said to have its classical exemplification in dream-interpretation. It means in fact that the dream is a falsification or lie; it not merely symbolises a successful outlet for tendencies that have hitherto been held in, but it represents such success as having been gained in the past, it turns previous defeats into victories. On this understanding, the aim of analysis is precise dating, is finding just what earlier events are now being symbolically denied. The fact that a suppressed impulse will regularly, and in various ways and directions, have striven to find outlet, permits it still to be maintained that different analyses are possible—there is not one but a whole series of falsifications or spurious successes; but this does not mean that any given one of them cannot be accurately traced from the dream. The Freudian doctrine is not, of course, entirely new; Nietzsche, for example, was expressing the same position in his epigram, "I did this, says Memory. I could not have done this, says Pride, and remains inexorable. Finally, Memory yields". But Freud is the first to have brought out with precision the character and range of the phenomena in question.

Part of the illumination given by Freudianism lies in its assimilation of diverse phenomena, its demonstration of the working of the same laws in such supposedly unconnected cases as dreams, forgetting, slips of the tongue and neurotic symptoms. But its initial impact was all the stronger from the fact that most of these things had been regarded as subject to no law whatever. Freud has illuminated human life and character by rendering intelligible what had been dismissed as accidental and unworthy of scientific consideration—and, once more, he has shown that the assumption of "meaninglessness" is itself part of the process of defence against unpalatable reality. It is, of course, the finding of significance in *dreams* that is the most striking example of Freud's opening up of the psychological field, and the most noteworthy point is that he has done so by emphasis on *activity*, by finding the working of psychical forces in what had been taken to be a mere picture unrolled on the mental screen.

Like all doctrines, Freudianism has its limitations. These are most apparent in the Freudian movement, since no school can quite rise to the inspiration of its founder. But in Freud himself the limitations are connected with the origin of the doctrine in the treatment of nervous disorders. The view that, because a theory has had a practical stimulus to its formation, it should have a practical outcome (an outcome other than *understanding* which is in its own way a part of human practice) is an example of that emphasis on externals against which revolutionary thinking is directed. The notion of "cure" has been one of the factors leading to the postulation of "the" interpretation of a dream or symptom, and in turn to the reinstatement of the division between normal and abnormal and to the ignoring of the universality of conflict. Freud himself avoids the extravagances of those of his followers who "settle" social conflicts in the manner of the consulting-room, who minister to the world as one great patient. But his later works certainly manifest a loss of philosophical force, a growth of "totalism", a lack of recognition of the fact that the denial of discontinuity does not mean the assertion of identity. The continuity of Man and Nature still leaves a distinction between human and non-human problems, does not merge them in one great problem or imply one great solution of all riddles.

The fact remains that Freud's work has given a tremendous impetus to rigorous thinking on human affairs, to the establishment of a real psychological science. His impact on present-day students is inevitably less than that on students of a generation ago; he has come to be taken for granted, to be treated with a certain complacency as contrasted with earlier enthusiasm and denunciation. But it is not true that "we are all Freudians now"; we can be Freudians only by hard thinking, by returning to Freud as a *classic* for the correction of current looseness. And even if no such "back to Freud" movement takes place, the Freudian revolution will not have petered out; Freudianism will remain as one of the determinants of the outlook of all critical thinkers.

NEGATIVE QUANTIFIERS

By A. N. PRIOR

"IF 'Φx' is an undetermined value of a certain propositional function, it will be *necessary* if the function is always true, *possible* if it is sometimes true, and *impossible* if it is never true." So writes Russell, in his *Introduction to Mathematical Philosophy.*[1] I do not wish to comment here on the propriety of this employment of the modal adjectives "necessary", "possible" and "impossible", but rather on the fact that although there are three ways of quantifying propositional functions mentioned here—"Φx always", "Φx sometimes" and "Φx never"—ordinary systems of quantification do not employ a separate symbol for the third; and I propose to investigate the consequences of removing this gap in our apparatus of quantificational symbolism. We may also be reminded of this gap by the definition of "some" as "Not all not", in *Principia Mathematica* 10.01. For if it is useful to have an abbreviation for "Not all not", may it not turn out to be sometimes useful to have also an abbreviation for the simpler "All not", and for that matter for "Not all"?

In putting this suggestion into effect, though referring constantly to the theses of *Principia Mathematica,* I shall not employ the symbolism of that work, but the more convenient symbolism of Professor Lukasiewicz,[2] his quantifiers "Πx" ("Whatever x may be") and "Σx" ("There is at least one x such that") being supplemented by the negative quantifiers

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[1] p. 165.

[2] That is, we shall use 'Np' for 'Not p', 'Kpq' for 'Both p and q', 'Apq' for 'Either p or q', 'Cpq' for 'If p then q', 'Epq' for 'If and only if p then q', 'Πxfx' for 'For all x, fx' and 'Σxfx' for 'For some x, fx'.

“Ωx”, “There is no x such that” and “Γx”, “Not every x is such that”,[3] “Ωx” being defined as “ΠxN” and “Γx” as “NΠx”, and “Σx” as in PM 10, as “NΠxN”. This suggests the following general recursive definition of “quantifier”: “Πx” is a quantifier, and if “Φx” is any quantifier, then so are “NΦx” and “ΦxN”. This gives us the infinite series of quantifiers “Φx”, “NΦx”, “ΦxN”, “NΦxN”, “NNΦx”, “ΦxNN”, “NΦxNN”, “NNΦxN”, “NNNΦx”, “ΦxNNN”, “NΦxNNN”, and so on; but it is easy to show that any quantifier past the first four of these is equivalent to one or the other of those four. The equivalence of “NNΦxfx” to “Φxfx” follows immediately from the ordinary law of double negation ENNpp (PM 4.13, converted in accordance with 4.21); and the equivalence of “ΦxNNfx” and “Φxfx” can be shown recursively as follows: If, for any quantifier Φ, EΦxNNfxΦxfx is a law, then so is ENΦxNNfxNΦxfx (for Epq always implies ENpNq), and so is EΦxNNNfxΦxNfx (for this follows by the substitution fx/Nfx in the supposed law EΦxNNfxΦxfx). Now it can be shown that EΠxNNfxΠxfx is a law. Therefore by what has just been proved, ENΠxNNfxNΠxfx and EΠxNNNfxΠxNfx are laws. And since ENΠxNNfxNΠxfx is a law so are ENNΠxNNfxNNΠxfx and ENΠxNNNfxNΠxNfx; and similarly with all the quantifiers which the definition of “quantifier” enables us successively to construct.[4]

We can now consider what additions to PM 10 and 11 are worth formulating with the aid of these new quantifiers. We shall find, in brief, that they enable us to formulate useful additions to what PM 10 has on the “movement of quantifiers” in complex matrices (i.e. such laws as 10.5, CΣxKfxgxKΣxfxΣxgx, “If something is both F and G, then both something is F and something is G"), and to its laws of subalternation (10.25, CΠxfxΣxfx, and 10.251, CΠxNfxNΠxfx) and equipollence (10.252, ENΣxfxΠxNfx, and 10.253, ENΠxfxΣxNfx); and to similar laws in PM 11 involving double and triple quantifications. We shall assign numbers to our new theses which extend the numeration of *Principia Mathematica.*

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[3] Ordinary speech has no one word for ‘Not every’, as it has ‘No’ for ‘Not any’ (much as ordinary speech, though it has ‘Neither’ for ‘Not either’, has no one word for ‘Not both’, and so nothing quite corresponding to Lukasiewicz’s ‘D’ or the Sheffer-Nicod ‘stroke’).

[4] We might compare the equivalence of all other quantifiers to Π, Σ, Ω or Γ, with the equivalence—as far as ordinary angular measurement is concerned—of all angles greater than 360° to angles between 0° and 360°.

10.7. Laws of Movement of Quantifiers.—Under this head PM has the following two equivalences:

10.22. EΠxKfxgxKΠxfxΠxgx
10.42. EAΣxfxΣxgxΣxAfxgx

("If and only if everything is both F and G then both everything is F and everything is G" and "If and only if either something is F or something is G then something is either F or G"); and the implications

10.5. CΣxKfxgxKΣxfxΣxgx
10.41. CAΠxfxΠxgxΠxAfxgx

("If something is both F and G then both something is F and something is G" and "If either everything is F or everything is G then everything is either F or G"). The introduction of Ω and Γ gives us the following new equivalences:

10.71. EΩxAfxgxKΩxfxΩxgx
10.72. EΓxKfxgxAΓxfxΓxgx

("If and only if nothing is either F or G then both nothing is F and nothing is G", "If and only if something is not both F and G then either something is not F or something is not G") and the following new implications:

10.73. CΩxAfxgxAΩxfxΩxgx
10.74. CAΩxfxΩxgxΩxKfxgx
10.75. CKΩxfxΩxgxΩxKfxgx (following from 10.71-73-74)
10.76. CΓxAfxgxKΓxfxΓxgx
10.77. CKΓxfxΓxgxΓxKfxgx
10.78. CΓxAfxgxAΓxfxΓxgx (following from 10.76-77-72).

The relations between the various compound and complex propositions involved in these formulae may be usefully exhibited in the following pair of hexagons (equivalent forms being given in brackets):

Affirmatives

(1) ΠxKfxgx
(KΠxfxΠxgx)
implies both
(2) AΠxfxΠxgx and (3) ΣxKfxgx
which implies which implies
(4) ΠxAfxgx (5) KΣxfxΣxgx
which both imply
(6) ΣxAfxgx
(AΣxfxΣxgx)

Negatives

(7) ΩxAfxgx
(KΩxfxΩxgx)
implies both
(8) AΩxfxΩxgx and (9) ΓxAfxgx
which implies which implies
(10) ΩxKfxgx (11) KΓxfxΓxgx
which both imply
(12) ΓxKfxgx
(AΓxfxΓxgx).

We shall give a full proof of 10.71, and abridged proofs of 10.72-73-74-76-77. Involved in the proof of 10.71 is the metalogical rule that in any law of the logic of propositions, we may replace the propositions entering into the law by matrices containing the variable x, and bind this variable by a universal quantifier prefixed to the whole law. Thus we may pass by this rule from Cpp to ΠxCfxfx, or from CEpqEqp to ΠxCEfxgxEgxfx. We shall call this rule RM. The premisses used in the proof of 10.71 are the following:

10.011. Ωx = ΠxN (Definition).
4.221. CEpqCEqrEpr.
4.561. ENApqKNpNq.
10.22. EΠxKfxgxKΠxfxΠxgx.
10.271. CΠxEfxgxEΠxfxΠxgx.

Here 10.22 and 10.271 are both theses of PM, and 4.221 and 4.561 are simple modifications of PM 4.22 (CKEpqEqrEpr) and 4.56 (EKNpNqNApq). This is the proof:[5]

4.561 X RM = 10.701.
10.701. ΠxENAfxgxKNfxNgx.
10.271 fx/NAfxgx, gx/KNfxNgx =
C 10.701 – 10.702.
10.702. EΠxNAfxgxΠxKNfxNgx.
10.22 fx/Nfx, gx/Ngx = 10.703.
10.703. EΠxKNfxNgxKΠxNfxΠxNgx.
4.221 p/ΠxNAfxgx, q/ΠxKNfxNgx,
r/KΠxNfxΠxNgx = C10.702 – C10.703 – 10.704.
10.704 EΠxNAfxgxKΠxNfxΠxNgx.
10.704 X 10.011 = 10.71.
10.71 EΩxAfxgxKΩxfxΩxgx.

But the process by which 10.71 is arrived at would be clear enough if we simply wrote down the following:

ΩxAfxgx = ΠxNAfxgx (Def. Ω) = ΠxKNfxNgx
(4.561 RM) = KΠxNfxΠxNgx (10.22) =
KΩxfxΩxgx (Def. Ω).

And the following proofs of the other numbers (an arrow replacing the "equals" sign where what is involved is an implication but not an equivalence) should also be clear enough:

10.72 ΓxKfxgx = ΣxNKfxgx = ΣxANfxNgx =
AΣxNfxΣxNgx = AΓxfxΓxgx.
10.73. ΩxAfxgx = KΩxfxΩxgx (10.71) → AΩxfxΩxgx.

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[5] In this proof 'F *α*/*β*' means 'F with *α* replaced by *β*'; 'FxR' means 'F with the rule or definition R applied to it'.

10.74. AΩxfxΩxgx = AΠxNfxΠxNgx → ΠxANfxNgx =
ΠxNKfxgx = ΩxKfxgx.

10.76. ΓxAfxgx = ΣxNAfxgx = ΣxKNfxNgx →
KΣxNfxΣxNgx = KΓxfxΓxgx.

10.77. KΓxfxΓxgx → AΓxfxΓxgx = ΓxKfxgx (10.72)

10.8. Theses holding for all Quantifiers.—In our proof of 10.71 we made use of PM 10.271, CΠxEfxgxEΠxfxΠxgx. PM also has the thesis CΠxEfxgxEΣxfxΣxgx (10.281), in which "Σ" replaces "Π" at two points in the preceding. By using "Ω" and "Γ" at the same two points, we obtain the following further true formulae:

10.81. CΠxEfxgxEΩxfxΩxgx

10.82. CΠxEfxgxEΓxfxΓxgx.

These state in effect that Ωxfx and Γxfx, as well as Πxfx and Σxfx, are what PM 20 calls "extensional functions of functions"—if either of them holds for any given function, it will hold equally for any other function with the same extension (any "formally equivalent" function). Using "Φx" to stand indifferently for any quantifier, we may assert 10.271, 10.281, 10.81 and 10.82 together in the single formula

10.83. CΠxEfxgxEΦxfxΦxgx.

We might prove this recursively from 10.271 as follows: If 10.83 holds for any quantifier Φx it can be shown to hold for NΦx, by the proof

ΠxEfxgx → EΦxfxΦxgx = ENΦxfxNΦxgx (PM 4.11).

It can also be shown to hold for ΦxN, by the proof

ΠxEfxgx = ΠxENfxNgx (PM 10.412) → EΦxNfxΦxNgx
(by the substitutions fx/Nfx, gx/Ngx in our supposed law 10.83).

But by 10.271, 10.83 holds for Πx; hence (by what has just been shown) for all the quantifiers successively constructed from Πx.

Other formulae which still hold when "Π" is replaced by "Σ", "Ω" and "Γ" are 10.32, EΠxEfxgxΠxEgxfx, and 10.412, EΠxEfxgxΠxENfxNgx. That is, we may affirm

10.84. EΣxEfxgxΣxEgxfx,
10.841. EΩxEfxgxΩxEgxfx,
10.842. EΓxEfxgxΓxEgxfx,

and so (taking 10.32 into account also)

10.85. EΦxEfxgxΦxEgxfx;

and also

10.86. EΣxEfxgxΣxENfxNgx
10.861. EΩxEfxgxΩxENfxNgx
10.862. EΓxEfxgxΓxENfxNgx,

and so (taking 10.412 into account also)

10.87. EΦxEfxgxΦxENfxNgx

The simplest way of proving this group of theses is perhaps by first proving the more general formulae 10.85 and 10.87 from 10.83, and then deriving the others from these by appropriate substitutions for "Φ". The proofs of 10.85 and 10.87 are as follows:

4.11. EEpqENpNq
4.21. EEpqEqp
10.83. CΠxEfxgxEΦxfxΦxgx
4.21 X RM = I.
I. ΠxEEfxgxEgxfx
10.83 fx/Efxgx, gx/Egxfx = C I – 10.85
10.85. EΦxEfxgxΦxEgxfx
4.11 X RM = II
II. ΠxEEfxgxENfxNgx
10.83 fx/Efxgx, gx/ENfxNgx = C II – 10.87
10.87. EΦxEfxgxΦxENfxNgx

Similar proofs using RM and 10.83 will enable us to pass from any law of the general form Epq (like 4.11 and 4.21) to the corresponding law of the form EΦxfxΦxgx, e.g. from Epp to EΦxfxΦxfx, from EApqAqp to EΦxAfxgxΦxAgxfx, etc.

10.9. Principles of Subalternation and Equipollence.—The laws of subalternation which may be obtained when "Ω" and "Γ" are introduced are as follows (I include the laws already in PM for symmetry and give them their PM numbers):

(1) Laws not involving "N", and relating two quantifiers:

10.25. CΠxfxΣxfx
10.91. CΩxfxΓxfx

(2) Laws involving one quantifier, but with an "N" in each component:

10.251. CΠxNfxNΠxfx   10.921 CNΣxfxΣxNfx
10.92. CΩxNfxNΩxfx   10.922. CNΓxfxΓxNfx.

(If everything is not-F then not everything is F; if nothing is not-F then not nothing-is-F; if not anything is F then something is not F; if not anything is-not F then something is-not not-F).

(3) Laws involving one quantifier, but with two N's in one component:

10.93. CΠxfxNΠxNfx   10.932. CNΣxNfxΣxfx
10.931. CΩxfxNΩxNfx   10.933. CNΓxNfxΓxfx

(If everything is F then not everything is not-F, etc.).

And the laws of equipollence:

(1) Laws exemplifying the medieval "first rule of equipollence", viz. that if "not" is introduced *before* a sign of quantity the result is equipollent to the *contradictory* of the original:

10.94. ENΠxfxΓxfx   10.942. ENΣxfxΩxfx
10.941. ENΓxfxΠxfx   10.943 ENΩxfxΣxfx.

(2) Laws exemplifying the medieval "second rule of equipollence", that if "not" is inserted *after* a sign of universality the result is equipollent to the *contrary* of the original, and the corresponding rule for subcontrary particulars:

10.95. EΠxNfxΩxfx   10.952. EΣxNfxΓxfx
10.951. EΩxNfxΠxfx   10.953. EΓxNfxΣxfx.

(3) Laws exemplifying the medieval "third rule of equipollence", that if "not" is inserted *both* before and after a sign of quantity the result is equipollent to the *subaltern* (i.e. either the subalternant universal or the subalternate particular) of the original:

10.96. ENΠxNfxΣxfx   10.962. ENΩxNfxΓxfx
10.961. ENΣxNfxΠxfx   10.963. ENΓxNfxΩxfx.

(4) Laws exemplifying the rule (not among the medieval three[6]) that if "not" is inserted *before* a sign of quantity the result is equipollent to the *subaltern* form with a "not" inserted *after* the sign of quantity (two of these are already in PM):

10.253. ENΠxfxΣxNfx   10.97. ENΩxfxΓxNfx
10.252. ENΣxfxΠxNfx   10.971. ENΓxfxΩxNfx.

11.8. Movement of Quantifiers in Multiply Quantified Propositions.—Before formulating the theses arising in this field, a decision must be made about the meaning to be attached to the forms "Ωxyfxy" and "Γxyfxy". In PM 11.01 the form corresponding in the PM symbolism to "Πxyfxy" is defined as an abbreviation for "ΠxΠyfxy", and in 11.03 "Σxyfxy" is similarly defined as short for "ΣxΣyfxy". This suggests that the form "Ωxyfxy" should be used as an abbreviation for "ΩxΩyfxy", and "Γxyfxy" as short for "ΓxΓyfxy". On the other hand, by analogy with the definition of "Ωxfx" as "ΠxNfx", and of "Γxfx" as "NΠxfx", we might define "Ωxyfxy" as "ΠxyNfxy" and "Γxyfxy" as "NΠxyfxy". In the case of "Σxyfxy" the analogous procedure of using the form as short for "NΠxyNfxy" would yield the same practical result as the actual PM definition of it as "ΣxΣyfxy". For the equivalence EΣxΣyfxyNΠxyNfxy can easily be shown, as by the following abbreviated proof:

ΣxΣyfxy = NΠxNNΠyNfxy = NΠxΠyNfxy = NΠxyNfxy.

But ΩxΩyfxy is *not* equivalent to ΠxyNfxy, as may be shown thus: ΩxΩyfxy = ΠxNΠyNfxy = ΠxΣyfxy; but ΠxyNfxy = ΠxΠyNfxy = ΠxNΣyfxy and ΠxΣyfxy and ΠxNΣyfxy are certainly not equivalent. Similarly ΓxΓyfxy is not equivalent to NΠxyfxy. For ΓxΓyfxy = NΠxNΠyfxy = ΣxΠyfxy, while NΠxyfxy = NΠxΠyfxy = ΣxNΠyfxy, and ΣxΠyfxy and ΣxNΠyfxy are again plainly not equivalent.

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[6] For the three medieval rules see Peter of Spain, *Summulae Logicales*, Bochenski's edition (Turin 1947), 1.25 and 1.27; also C. S. Pierce, *Collected Papers* 2.584, fourth mnemonic.

The solution adopted here will be to define "Ωxyfxy" and "Γxyfxy" in the second way indicated, i.e. as being respectively abbreviations for "ΠxyNfxy" and "NΠxyfxy". This is in accordance with the most natural way of reading these forms, namely as quantifying ordered couples. Thus reading "Ωxyfxy" as "There is no pair of objects, x, y such that x is F to y", the meaning is "Nothing is F to anything" ("Everything is not-F-to-anything"), ΠxyNfxy, not "Nothing is F to nothing", ΩxΩyfxy. We may now incorporate the PM laws of movement for doubly quantified propositions in the following enlarged set of theses:

11.2. EΠxyfxyΠyxfxy
11.23. EΣxyfxyΣyxfxy
11.81. EΩxyfxyΩyxfxy
11.82. EΓxyfxyΓyxfxy
11.26. CΣxΠyfxyΠyΣxfxy
11.83. CΠxΩyfxyΩyΠxfxy
11.84. CΠxΓyfxyΓyΠxfxy
11.85. CΩxΣyfxyΣyΩxfxy
11.86. CΓxΣyfxyΣyΓxfxy
11.87. CΩxΓyfxyΓyΩxfxy.

The proof of 11.83 is as follows:

11.83. ΠxΩyfxy = ΠxΠyNfxy = ΠyΠxNfxy →
ΠyΣxNfxy = ΠyNΠxfxy = ΩyΠxfxy

The other proofs are similar in character.

It may be noted that 11.81-82 would not be true if "Ωxyfxy" were defined as "ΩxΩyfxy" and "Γxyfxy" similarly. For ΩxΩyfxy and ΩyΩxfxy are not equivalent. For example, "nothing has nothing that it loves" (i.e. "Everything loves something") is not equivalent to "Nothing has nothing that loves it" (i.e. "Everything is loved by something"). On the other hand, the associative principle that appears in PM as 11.27, EΣxyΣzfxyzΣxΣyzfxyz, and that is implicitly asserted for Π also in 11.21, does hold for Ω and Γ when "Ωxy" is interpreted as short for "ΩxΩy", and similarly with Γ, but when "Ωxy" and "Γxy" are interpreted as here recommended, the relevant equivalences are depressed to the implications

11.88. $C\Omega xy\Omega zfxyz\Omega x\Omega yzfxyz$

11.89. $C\Gamma x\Gamma yzfxyz\Gamma xy\Gamma zfxyz$

The proof of 11.88 is as follows:

$$\Omega xy\Omega zfxyz = \Pi xyN\Pi zNfxyz = \Pi xy\Sigma zfxyz = \Pi x\Pi y\Sigma zfxyz \rightarrow$$
$$\Pi x\Sigma y\Sigma zfxyz = \Pi x\Sigma yzfxyz = \Pi xN\Pi yzNfxyz = \Omega x\Omega yzfxyz.$$

The fact that ΩxΩyzfxyz (interpreted our way) does not imply ΩxyΩzfxyz, though it is implied by it, may be brought out by an example. Let the range of the variables x, y and z be that of the positive integers, and let fxy be "$x = y - z$". ΩxΩyzfxyz will then become the true proposition "No positive integer x has no pair of positive integers y, z, such that $x = y - z$" (i.e. every positive integer may figure as the difference between some two positive integers); but ΩxyΩzfxyz becomes the false proposition "No pair of positive integers x, y has no positive integer z such that $x = y - z$". (This is false because you only have to let x be greater than y to have a pair of positive integers which has no positive integer z for which $x = y - z$.)

11.9. Equipollences of Doubly Quantified Forms.—In medieval logic, the theory of *aequipollentia* concerned the introduction of a negative particle at various points in a proposition (cf. 10.9). The introduction of a secondary quantification provides further points at which a negative particle may be put, and this possibility is exploited in PM 11.5 (EΣxNΠyfxyNΠxyfxy; EΣxNΠyfxyΣxyNfxy) and 11.51 (EΣxΠyfxyNΠxΣyNfxy). The addition of negative quantifiers to the usual two affirmative ones opens up the further possibility of introducing the negation into the proposition in a new way—not by means of "not" but by means of "no" and "not-all". Medieval logicians were not entirely unaware of the possibility of extending the theory of equipollence in this direction; for Peter of Spain gives an example of the form EΩxΩyfxyΠxΣyfxy which he proves as follows:

ΩxΩyfxy = ΠxNΩyfxy (10.95) = ΠxΣyfxy (10.943 RM). What he says is that "Nothing is nothing" is equivalent to "Everything is something", for by the second medieval rule of equipollence (see 10.9) the former is equivalent to "Everything is not-nothing", and by the first medieval rule "Not no" = "Some". He gives the rule that in all such cases the first sign of universal negation is "equipollent to its contrary" and the second to its contradictory, i.e. we may simultaneously change the first "Ω" to "Π" and the second to "Σ".[7] This rule can in fact be extended to all cases of double quantification whatsoever, substituting "subcontrary" for "contrary" where the first quantifier is particular.[8] For the "contrary" or "subcontrary" quantifier to any given quantifier Φx is that which has the same force as ΦxN, while the "contradictory" quantifier to Φx is that which has the force of NΦx; so the changes which the rule mentions (of the first quantifier to its contrary or subcontrary, and the second to its contradictory) will always turn the given form ΦxΨyfxy into a form equivalent to ΦxNNΨyfxy, and the equivalence EΦxNNΨyfxyΦxΨyfxy may be derived simply by the substitution of Ψyfxy for fx in the law EΦxNNfxΦxfx. This rule gives us the following theses, all of which may also be proved in the same general manner as Peter proves the first:

11.91. EΩxΩyfxyΠxΣyfxy
11.92. EΓxΓyfxyΣxΠyfxy
11.93. EΠxΩyfxyΩxΣyfxy
11.94. EΣxΓyfxyΓxΠyfxy
11.95. EΠxΓyfxyΩxΠyfxy
11.96. EΣxΩyfxyΓxΣyfxy
11.97. EΩxΓyfxyΠxyfxy
11.98. EΓxΩyfxyΣxyfxy

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[7] op. cit., 1.26.

[8] Peter of Spain was unable to formulate his principle with this generality because he used words rather than symbols, and Latin, like English, has single words corresponding only to 'Π', 'Σ' and 'Ω', expressing 'Γ' by 'Some' and the particle 'not'. *Nullus* alone has both its contrary and its contradictory expressed in ordinary speech by special quantifiers.

From the last two of these we may draw as corollaries, in view of the equivalence of "Πxy" to "Πyx" (11.2) and of "Σxy" to "Σyx" (11.23), the following:

11.971. EΩxΓyfxyΩyΓxfxy
11.972. EΓxΩyfxyΓyΩxfxy.

(Proof of 11.971: ΩxΓyfxy = ΠxΠyfxy = ΠyΠxfxy = ΩyΓxfxy.)

Each of the forms Ωxyfxy and Γxyfxy may be "opened out" into a doubly quantified form in two distinct ways. In each of the two ways, one of the two new quantifiers is identical with the original single one, while the other is different; and if the different quantifier is placed first, it is the contrary (or subcontrary) quantifier to the original; and if placed second, the contradictory quantifier. Thus we have the equipollences:

11.991. EΩxyfxyΠxΩyfxy
11.992. EΩxyfxyΩxΣyfxy
11.993. EΓxyfxyΣxΓyfxy
11.994. EΓxyfxyΓxΠyfxy.

(Proof of 11.991: Ωxyfxy = ΠxyNfxy = ΠxΠyNfxy = ΠxΩyfxy.[9]) From each of these we may draw a corollary in the light of the equivalence of "Ωxy" to "Ωyx" (11.81) and of "Γxy" to "Γyx" (11.82), thus:

11.995. EΠxΩyfxyΠyΩxfxy.
11.996. EΩxΣyfxyΩyΣxfxy.
11.997. EΣxΓyfxyΣyΓxfxy.
11.998. EΓxΠyfxyΓyΠxfxy.

(Proof of 11.995: ΠxΩyfxy = ΠxΠyNfxy = ΠyΠxNfxy = ΠyΩxfxy.)

The main results of this and the preceding section may be expressed by the following pair of hexagons, equipollent forms being given in brackets:

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[9] A general proof of our rule is as follows: If Φ is a negative quantifier, then for the contradictory quantifier NΦ it will hold that (NΦ)xy = (NΦ)x(NΦ)y. Hence Φxy = NNΦxy = NNΦxNΦy = ΦxNΦy = (ΦxN)Φy = Φx(NΦy).

Affirmatives

(1) Πxyfxy
(ΩxΓyfxy, ΩyΓxfxy)
implies both

(2) ΣyΠxfxy
(ΓyΓxfxy)
which implies
(4) ΠxΣyfxy
(ΩxΩyfxy)

and

(3) ΣxΠyfxy
(ΓxΓyfxy)
which implies
(5) ΠyΓxfxy
(ΩyΩxfxy)

which both imply
(6) Σxyfxy
(ΓxΩyfxy, ΓyΩxfxy)

Negatives

(7) Ωxyfxy
(ΠxΩyfxy, ΠyΩxfxy,
ΩxΣyfxy, ΩyΣxfxy)
implies both

(8) ΣyΩxfxy
(ΓyΣxfxy)
which implies
(10) ΠxΓyfxy
(ΩxΠyfxy)

and

(9) ΣxΩyfxy
(ΓxΣyfxy)
which implies
(11) ΠyΓxfxy
(ΩyΠxfxy)

which both imply
(12) Γxyfxy
(ΣxΓyfxy, ΣyΓxfxy,
ΓxΠyfxy, ΓyΠxfxy)

The numbered forms may be expressed verbally as follows, letting f = "loves":

(1) Everything loves everything (Nothing has anything that it does not love, Nothing has anything that does not love it).

(2) Something is loved by everything (Something has not anything that does not love it).

(3) Something loves everything (Something has not anything that it does not love).

(4) Everything loves something (Nothing loves nothing).

(5) Everything is loved by something (Nothing has nothing that loves it).

(6) Something loves something (Something has not nothing that it loves, Something has not nothing that loves it).

(7) There are no pairs such that one member loves the other (Everything loves nothing, Everything is loved by nothing, Nothing loves anything, Nothing is loved by anything).

(8) Something has nothing that loves it (Something has not anything that loves it).

(9) Something has nothing that it loves (Something has not anything that it loves).

(10) Everything has something that it does not love (Nothing loves everything).

(11) Everything has something that does not love it (Nothing is loved by everything).

(12) Some pair of objects is not such that one member loves the other (Something has something that it does not love, Something has something that does not love it, Something does-not love-everything, Something is-not loved-by-everything).

In forms with one quantifier only, the replacement of this by the contradictory quantifier gives the contradictory proposition; in forms with two quantifiers, the replacement of the *first* by the contradictory quantifier, the other being left unchanged, gives the contradictory proposition. Thus (1) and (12), (2) and (11), (3) and (10), (4) and (9), (5) and (8), and (6) and (7) are pairs of contradictories.[10] The same numbers are contradictories in the hexagons in 10.7. In both sets of cases, the proofs are for the most part simple substitutions in 10.94-943, eked out in some cases in 10.7 by de Morgan's Laws. With the doubly quantified forms, an alternative contradictory may be obtained by replacing the first quantifier by its subaltern and the second by its contradictory, e.g. ΠxΓyfxy is contradicted by ΣxΠyfxy as well as by ΓxΓyfxy. Proof: The subaltern quantifier to Φx is always equivalent to NΦxN (10.96-963), so that the replacements indicated turn ΦxΨyfxy into an equivalent of NΦxNNΨyfxy, and this is equivalent to NΦxΨyfxy.

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[10] In short, (n) and (13-n) are pair of contradictories throughout.

Yet another set of equivalent contradictories (not represented in our hexagon) may be obtained by the rule: Replace each quantifier by the subaltern quantifier, and insert "N" between the quantifier and the expression quantified. For since the subaltern quantifier to Φx is always equivalent to NΦxN, this transformation will turn ΦxΨy into NΦxNNΨyNN, and this is equivalent to NΦxΨy. For example, the contradictory of ΠxΣyfxy may be given not only as ΓxΣyfxy and as ΣxΩyfxy but also as "ΣxΠyNfxy" ("Everything is a lover of something" is contradicted not only by "Something does not love anything" and "Something loves nothing" but also by "Something is a non-lover of everything"). PM 11.51, EΣxΠyfxyNΠxΣyNfxy, is almost an example of this, since it yields, by the law EEpNqENpq, ENΣxΠyfxyΠxΣyNfxy. This rule may be extended to any number of quantifiers, being proved in the same way in each case. Thus the contradictory of ΠxΣyΣzfxyz may be given as ΣxΠyΠzNfxyz. ("Everything is a giver of something to something" is contradicted by "Something is a non-giver of everything to everything", i.e. gives nothing to anything).

Note on the Interpretation of our Symbols.—All the laws of quantification considered in this paper will hold not only in a "Russellian" type of logic but also in an "Andersonian" type of logic in which (*a*) the completely general term "thing" is replaced by descriptive terms like "man", "horse", etc., and (*b*) empty terms like "dragon", "mermaid", etc., are not admitted. If our formulae are being used in such a logic, their verbal interpretations will, of course, have to be altered. For example, our doubly quantified form (1) will have to be read, not as "Everything loves everything" but as "Every X loves every Y", where "X" and "Y" might stand for "man" and "woman" respectively ("Every man loves every woman" is equivalent to "No man has any woman whom he does not love", implies "Some man loves every woman", which in turn implies, though it is not implied by, "Every woman has some man who loves her"; and so on). Similarly (1) in 10.7 will have to be read, not as "Everything is both F and G", but as "Every X is both F and G" (e.g. "Every man is both a lover of Sally and a lover of Sue").

Peter of Spain, we may note, illustrates the rule symbolised by our 11.91 not only by the Russellian equipollence "Nothing is nothing = Everything is something" but also by the (relatively) Andersonian equipollence "No man is no animal = Every man is some animal"; and if our symbols are given the Andersonian interpretation we may regard Peter's reminder, in connection with the "fallacy of division", that "Every animal is either rational or irrational" is not equivalent to "Either every animal is rational or every animal is irrational" [11] as an illustration of the non-reversibility of the implication 10.41.

POSTCRIPT.—While the above was in the press, I discovered, from a review by Church in the *Journal of Symbolic Logic* (Vol. XIII, p. 55), that C. H. Langford had a short paper discussing negative quantifiers in the *Bulletin of the American Mathematical Society* for November-December, 1926. Thanks to the kindness of Professor H. G. Forder I have been able to see a copy of this paper, and have found that there is very little overlapping between Langford's material and that presented here; though Langford too notes that the quantifier "For no x" is non-associative (p. 699 Cf. our 11.88).

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[11] Peter of Spain, op. cit. 7.29.

REVIEWS

THE PHILOSOPHY OF HENRY JAMES, SR. By Frederic Harold Young. (Bookman Associates, New York, 1951.) Price, $4.50.

This review must begin with a confession. Before I opened this work, I knew, by synthetic *a priori* argument, that William and Henry James must have had a father, and I surmised, from the achievement of the sons, that the father was of some ability and had taken some pains with their education. But I must admit that I did not know who he was, and still less that he was a considerable thinker in his own right, with a literary style of which either of his famous sons might well have been proud. To have made this discovery is a delight; and it is a double delight to have made it in the company of Professor Young. For his book is exactly what a thinker's book about another thinker ought to be. It is to the last degree scholarly; its footnotes, bibliography and production are impeccable; it is written with affection, yet with detachment; it displays a detailed and comprehensive, but completely assimilated knowledge of the field; it is sparing of critical comment, but what there is is right on the nail; it is inspired by a genuine interest in what its subject was trying to do; and it is itself written in a masterly and flexible style which in its clarity and urbanity provides a suitable foil for the sharp-shooting knock-down metaphorical style of James himself. It would be difficult for any historical figure to find a better compère. If, in what follows, the emphasis is not so much on Professor Young's achievement as on the work and thought of James, that is itself a tribute to the book, which is so excellent that it leaves no room for argument about itself, and directs all our attention to what it is about. Would that this could be said more often!

Indeed there is only one possible criticism: that the meticulous scholarship, the theological and philosophical knowledge, the mature and satisfying completeness of this work have been lavished on an object not quite worthy of it. Whether this is so or not must be judged from the record.

Henry James, Sr. (1811-1882), was the son of a Southern Irish Presbyterian who made a fortune from investments in the once famous Erie Canal. Vigorous, acute, pugnacious, high-principled, and passionately devoted to theologico-philosophical construction and disputation, he never had to make a living: and when compunctions about Calvinism took him out of the Princeton theological seminary before he completed his training as a minister, he was able to devote his life to his intellectual interests, studying and lecturing as and when he pleased. Whether this freedom from irksome obligations was altogether a good thing for a picturesque and overflowing and undisciplined mind may well be doubted. The advantage of curricula and professional duties is that they force one to pay some attention to the standard authorities. James, who read widely and indiscreetly, was dominated not by the masters in philosophy and theology, but by three successive eccentrics who impressed him early in life: Sandeman, Swedenborg and Fourier. Much of his life's work consisted in compounding a metaphysic of creation out of this extravagant material. Considering his wilful choice of mentors, the result is perhaps more sensible than might have been expected; and, to be fair, some of the credit is due to Swedenborg, who, as Professor Young shows, had a sensible side to him, too little regarded by the Swedenborgians; but the whole method of building a philosophical theology by synthesizing a trio of hyper-sectarian oddities would appear to be perverse and precarious. Years later, when William James gave his Gifford Lectures on the *Varieties of Religious Experience*, he showed the same curious penchant for the wilder sort of sectarian literature; but this deflection of judgment in the son affected mainly his choice of examples, while in the father it conditions the whole enterprise.

Yet it is impossible to read Professor Young's book and not to be impressed with a certain greatness in James. It is partly his superb command of the English language; but that in itself is the reflexion of a remarkable personality. Sometimes his thoughts appear to be more interesting as the record of such a personality than they are in themselves. Yet even here (such were his vitality and his unexpectedness) we can never be sure. Every now and again a thought emerges which gives a fresh slant to the perennial problems of philosophical theology. Philosophers will no doubt be disconcerted by his lack of system and his shifting vocabulary; but he has a remarkable gift of setting the problem in an original way; a gift which more systematic thinkers have too often been conspicuously denied.

The problems with which James concerned himself are the problems of a theologian: more particularly, creation and redemption. But James considered himself, not a theologian, but a philosopher; and his manner of treating his problems gives the claim a certain plausibility. It is never easy to say where theology ends and philosophy begins: but, as a matter of history, it may broadly be said that theology is an attempt to systematize a historic revelation, while philosophy, if it deals with revelation at all, deals with it as symbolic of a truth more accurately expressible in general terms. On this showing James was certainly a philosopher. No one was ever more emphatic about the need for revelation; and no one more consistently attempted to refine revelation into cosmic process. For example, creation he regarded not as temporal but as constitutive: "When I say God created me, I suppose myself already formed or existing" (p. 126); and while *The* Christ (i.e. that there should *be* an incarnation in the *sort* of person that Jesus was) was all-important for him, he has little to say about the *particular* incarnation in the historic Jesus. This resolution of history into symbol was characteristic of the Gnostic mood in the early Christian Church; and James, though not a patristic scholar, moved in a distinctly Gnostic atmosphere. It is to be noted in passing that the Gnostics shared the neo-Platonic conviction that God is vouchsafed primarily to an *élite* of *illuminati.* This is not at all what James wants: the descent of God into the world is a theme which he elaborates and even exaggerates. There is here the beginning of a breach between his conception of philosophy and the philosophy which he conceives.

James's philosophy, as we shall now call it, is progressive and dialectical: the account of reality is the account of a movement. There is the downward movement of creation, and the upward movement of redemption. It starts from the side of God; and as a contemporary reviewer remarked (p. 117), part of the difficulty in understanding James is that he considers creation from the angle of the creator. The whole dialectical movement can be summed up in the words of Irenaeus: "God became man that man might become God". It is as "God becoming man" that James considers divine creation.

The traditional problem of creation James treats lightly, and even dodges. What he is interested in is the creation of souls; indeed, it would make little difference to his argument if the world had existed from all eternity. Creation is a "wholly spiritual operation", "executed by God strictly within the sphere of 'affection and thought' " (p. 127). Souls are created when God enters into them. We write "enters" in order not to be extravagant; but "turns into them" might not be too vivid a rendering of the author's meaning. The movement in which souls are created is called a "movement of humiliation" (p. 133); in a sense, the creatures *are* the Creator, humiliated; they are distinguished from Him not substantially, but dialectically, as a new phase of His existence. It is not surprising that William wrote to his father from Berlin asking how he was going to avoid pantheism, or that the destiny of James's God was to be merged in the "divine community".

At the same time, this most unorthodox venture is characteristically poised on an orthodox truism. For God is love, and there can be no love greater than to dissolve into those who are loved (cf. p. 175).

We are now at the bottom of the cycle, and await the return. The creature is indeed separate, and it is his separateness which makes him a creature: "he must be absolutely and unchangeably himself" (p. 128). Nevertheless, "selfhood in man is not absolute, but only finite and conditional" (p. 174): "it is rigidly dependent upon the equilibrium of two opposing forces" (ibid). These forces are: good and evil (moral), truth and falsity (intellectual), and celestial and infernal (spiritual): the second of each pair representing the claim to self-sufficiency, and the first the sense of our dependence on the common creative force from which each of us is compounded. Thus the self-standingness which is the mark of the creation is, equally, his "original sin" (p. 225): a sin which God takes upon Himself in order to bring the creature back to Him—or, rather, to resume His advance with the creature re-absorbed.

At this stage, it might have been expected that the re-absorption would mean the reassembling of the fragmented God, with the creatures mystically dissolved in Him. But that would mean that redemption could only be effected beyond the world, and James took incarnation to mean the transference of God into the world. The development he foresaw was dialectical: the overcoming of the sin of separateness in a free society. It is here that he is indebted to Fourier, whose "associationist" brand of Socialism was, during the 1840's, almost a live option, and gave rise to many sporadic experiments in communal living, including, as James was well aware, the famous Brook Farm experiment in Massachusetts. Not that James was in any way involved in them: he was far too much of an individualist for that, and in any case he had a lively sense of the difference between immediate projects (in which he was neither expert nor interested) and the apocalypse over the horizon. But he caught from Fourier the intimation that only free association can combine the virtues of solidarity and liberty, and attached it, as the final phase of his dialectic, to his philosophy of God and man. The God who has been splintered into human individuals reassembles as the Divine Community. "Creation, spiritually regarded, means, not any stupid and brutal event in space and time, transcending human nature and antedating human history, but a most real and authentic life of God identical with human nature and consubstantial with human history" (p. 309).

The trouble about transferring the city of God to the world is that the cities of the world, all of them, will preen themselves as the rock from which God is hewn. Any theist who slurs the traditional antithesis, as James does, prepares the way for the social idolatry in which the sense of a God other than society disappears. It is only fair to James to note that he is thinking of *perfected* society (he dismisses both Church and State with zest and contumely), that he is wholly out of sympathy with Comte's "religion of humanity", as lacking in the authentic note of redemption (p. 306), and that his "divine community" is one in which men associate freely and do not exercise authority on one another, so that it will never be afflicted by the apotheosis of power. But it does, and it is meant to, overcome the antithesis between the divine and the human. The moment that step is taken, with whatever genuineness of religious feeling, the way is open for the innumerable modernisms in which the name of God is attached to whatever men of reasonably good will may happen to feel inclined to.

And yet it would be unjust to slur James's sense of divine election. It is true that he leans to a kind of "universalism", i.e. that, sooner or later, *all* men are elected; but he is utterly convinced that men move to their salvation only through the Godhead in them. Early in life he learnt from the Sandemanian heresy, not only that every man is a priest (he objected on principle to a specialized ministry) but also to spurn, in an even super-Calvinist style, the least suggestion of salvation through merit. He was, in consequence, despite the conventional blamelessness of his ordinary life, flagrantly antinomian in his sentiments. He had no use for "morality", which he belabours with all the energy of William Blake (whom he definitely regarded as a kindred spirit). Man is saved by what he is, not by what he reluctantly makes himself do. And as what he is is divinity disintegrated, it is the divine stirrings in his soul, feeling their way beyond the disintegration, which are the springs of his redemption. It is significant that James admires the artist as much as he dislikes the moralist, precisely because "he obeys his own internal taste or attraction, uncontrolled by necessity or duty" (p. 185). He believed that the God in us is expressed primarily in our spontaneity: that is to say, not on what we plan, but on what happens through us.

It will be noticed that in all this James writes only of the creation and redemption of spirits; and the question may be asked, What of nature? The orthodox doctrine is that nature is created. James dismisses it as "very silly": "the whole and sole function of nature is to constitute that supposititious realm of uncreation, not-being, out of which man is logically held to be delivered by his creation" (pp. 152-3).

The word "suppositious" is instructive. Nature is not really like that; it is, according to one version, mind-dependent in the idealist fashion, and, according to a better supported version, itself composed of infinitesimal spirits. It is therefore an ideal limit to the downward thrust of creation, by facing towards which man obtains consciousness of himself as a separate existence and thus obtains his identity. It thus serves as a "springboard" from which Man leaps to his upward movement; and yet not a real or substantial one, for it operates solely on the imagination, and a mere belief in it would give us all that James requires of it. He talks about nature constantly and with animation, but is totally uninterested in nature except as stage-scenery for the human drama. The effect of thus bringing everything to the side of spirit will be seen presently.

It is possible that this review does an injustice to James in attempting to present him doggedly and coherently: for his peculiar excellence lies not in the system, but in his capacity to suggest and stimulate. There may be some significance in his never having produced a *magnum opus.* It may have been a sense of his own strength and weakness which induced him to express his heart in lectures, reviews, pamphlets and public controversies. He was a sprinter by instinct: it might seem to be the wrong sort of flattery to appraise him as a miler. But Professor Young has shown us that the thought which he preferred to express in fragments was very far from being fragmentary. It was in many ways too abundant to be wholly consistent, and too full of grand gestures and loud asides to be judged by its skeleton; but there is a central theme, to which he continually returns from his exhilarating and exploratory digressions: a theme which it is a chief merit in Professor Young to have discerned above a truly tropical undergrowth of prolific suggestions.

To discuss James's theme in detail would exceed the limits of a review, and it has seemed preferable rather to elucidate it, so that readers can proceed to reflect on it themselves. The comment which it specially invites is that specific revelations cannot be generalized without wholly changing their character. James is so possessed and enthralled by the themes of incarnation and redemption that he spills them extravagantly over the scheme of things, and thinks it pusillanimous and restrictive to contain them in their original channels. But a flood is not a substitute for irrigation. In transferring the whole nature of God from its original transcendence into the community of souls, he is left with a God who *is* the community of souls. The conclusion of the dialectical process thus negatives not merely the negation of the thesis, but the thesis itself. The God who was before incarnation *ceases to be* in the process of incarnation. He does not reveal himself; He undergoes a substantial mutation. Moreover, even the swing of the pendulum does not restore Him. From the moment of creative "humiliation", He remains irrecoverably incarnate. To be wholly embodied in a fraternal community is to be no less wholly embodied than to be wholly embodied in self-standing individuals.

Yet if creation and redemption are to be movements *of* the world and not movements produced by God *in* the world there is no help for it. Unless the concepts of creation and redemption are attached to specific historical events, James's conclusion is the only possible one. But of those who find this conclusion distasteful, let us ask whether creation and incarnation are the same thing: for according to James they are and must be. Surely incarnation cannot *replace* creation, but is the descent of what is uncreated, and remains uncreated, into a world which is created, and remains created. And here we may revert to James's treatment of nature. A transcendent God and an authentic nature go together; the merging of God in the world confuses both of them. If nature were created, and thereafter were an other to its creator, creation and incarnation *would* be distinguished, as orthodox theological thinkers suppose. James's lack of interest in nature as such is both a cause and a consequence of his further entanglements. The worst of them, as we have seen, is the proposed deification of humanity, or to be more accurate, of *redeemed* humanity. The reservation is not unimportant; it means that the date at which man becomes God is at least postponed; and it means that man becomes God not because there is no God (as Comte and Feuerbach maintained), but because there is a God. But there is the same transference of religious emotion; and the ultimate idolatry is all the more striking for the utter lack of anything approaching a cynical undertone.

Perhaps, as Professor Young suggests, it takes an acquaintance with the Greek Fathers or the Greek Orthodox Church to interpret James's peculiar emphasis with the necessary understanding, and Western Christendom, dominated by St. Augustine in both its Catholic and its Protestant divisions, was not his proper setting. But, while making this plea for our sympathies, and extending his own to the uttermost, Professor Young shows by incidental comment that he sees the difficulties. (For example, he quotes as "penetrating" a critical letter to James from the English Swedenborgian, J. J. G. Wilkinson: "Christ seems to disappear into humanity, as God has disappeared in Christ; and Man is all in all" (p. 313).) It is therefore all the more admirable in him that he never sits in judgment, throws himself without reserve into making James acceptable to his readers, and allows himself only an occasional hint of his private reservations. His tactics at the end of his decisive chapter (Ch. XI), in which he delicately insinuates his doubts by quoting Wilkinson, and proceeds to make amends to James by printing verbatim two of his grandest, most exuberant, and most apocalyptic purple passages, is a masterpiece of comprehending self-effacement. He has given us a truly admirable sustained interpretative study, and, as those who are better able to judge would assuredly add, a notable contribution to the study of American intellectual history.

A. BOYCE GIBSON.

RELIGIOUS FAITH, LANGUAGE AND KNOWLEDGE. By Ben F. Kimpel, Professor of Philosophy, Drew University. (Philosophical Library, 1952.) Price, $2.75.

This is not a great book; in its writing and arrangement it is not even a good book: but it is an apt and timely book, and if some of its moments of insight had been isolated and reassembled, if the argument had been more consecutive, and if it had been rewritten in plainer English, it might have been a book of importance. Even as it is, it raises all the main issues for a major discussion on Religion and Contemporary Philosophy, and supplies some hints towards a solution. Those who are interested in this topic should certainly read it for the stimulus which it will provide; though they will probably agree at the end of it all that there is more to be said.

The main object is to support religion by analysing the defects of an attitude believed to be hostile to religion. This attitude, which Professor Kimpel calls "empiricism", is that which confines knowledge to "experience". He finds it not merely in Hume and the contemporary language-philosophers, but also in Kant, Descartes, Schleiermacher, and Rudolf Otto; the term "empiricist" is thus extended far beyond its normal boundaries, and theistic philosophers and theologians are included among its victims. What distinguishes the "empiricist" in his various disguises is that his analysis of "experience" contains no reference to the objects of experience; and the opposite of "empirical analysis" is "realistic analysis", which insists from the start on discriminating experienc*ing* from the experienc*ed.* This definition being allowed him, there is little difficulty in showing that a pure "empiricism" cannot interpret "experience", and that theologies which substitute the experience of God for the presence of God are an offence to religion.

It is a distinct merit of this classification that it shows the strongly idealist affinities of the current language-philosophy. The point about this philosophy is that it will not allow the referential function of language: language is treated as language *and nothing else*; in analysing it we analyse, at the best, our *view* of the situation, and do not approach the situation itself. This consideration applies not only to the stricter sect of Logical Positivists, but also (as Professor Passmore pointed out in this JOURNAL, December 1952) to their gentler and more agnostic brethren, the "logical analysts" of contemporary Oxford and Cambridge: the intrusive "we" of their conventional discourse makes its object for ever inaccessible. If to be an "empiricist" means to remain bogged in the average experience of one's time, there is no doubt whatever that our fashionable contemporaries have earned the title.

Moreover, Professor Kimpel gives good reasons for declining to be thus constricted. He points out that language and symbols generally may be considered either in themselves or for what they stand for; and to say that what is known is *merely* language or *merely* symbol is, in many cases at least, to neglect its function of referring to something else ("the sense of a symbol is the reality it designates", p. 102). It is not empirical, but dogmatic, to stifle the "intentional" aspect of the data. The "empiricist" who stops short on their surface supplements his experience with a theory, just as surely as the "realist" who acknowledges a reality designated. But, as he does not understand what he is doing, he is more likely to be deceived than the "realist", who does.

So far, Professor Kimpel has a strong case. But the object of this analysis is to support the claims of religion, and here the issues become confused. Two points may be selected by way of illustration.

In the first place, perhaps because of the more militant character of "logical analysis" in the United States, he misses an important distinction between analysts and positivists proper in their respective attitudes to religion. The positivist reduction of religion to private feelings and exclamations is deprecated by many "logical analysts", who are perfectly prepared to approve and even to practise religion outside the orbit of their philosophy. There are reasons for finding this position difficult, but they are not those used by Professor Kimpel in his debate with the positivists. And he cannot afford to overlook it, because, unless he can deal with it, it is a standing refutation of his view that "empiricism" is intrinsically anti-theological. The worst one can say is that it is non-theological.

And that leads to the second point. Being justly impressed by the ambiguities of the word "experience", Professor Kimpel is unwilling, when it comes to religion, to accept the experience-of-God as part of the data. Instead, he hunts for God among a host of presuppositions, assumptions, and projections, and contents himself with showing that religion, in this respect, is no worse off than science: in both cases, it is reasonable to suppose that the experience points to that which it symbolizes. And this analogy plays havoc with his discussion of religious faith, which is not at all like a scientific hypothesis. The scientist knows that his hypothesis *is* a hypothesis: the believer knows that God is *not* a hypothesis. Faith is not intelligent anticipation; it is a fiery avowal of trust and constancy. Apologetic comparisons do nothing to make it more credible; in fact, because they do not hold, they provoke a criticism which is as just as it is needless.

It should now be clear that while much of Professor Kimpel's philosophical groundwork is sound, it needs to be supplemented by a keener appreciation of religious faith. Seeing that he writes in support of faith, it is impossible to doubt that he has experienced it; all we ask is that he should confront us with what, as a worshipper, he knows, and not with an academic simulacrum. "Religious faith", he writes (p. 132), "is a point of view with which some experiences are interpreted." It is safe to say that no British writer could match this masterpiece of American understatement. The late Lord Lindsay of Birker used to quote with amusement the invocation of a Scots minister trained in philosophy: "O Thou who art our ultimate hypothesis and our everlasting hope"—the point of the joke being the incongruous jangle of science and prophecy. It is to be feared—and it would be a grave defect in a philosopher writing about religion—that Professor Kimpel would not think it at all funny.

But—and this must be repeated—his book is well timed, and admirably fitted to provoke discussion on the right issues. From this point of view, it is thoroughly to be commended.

A. BOYCE GIBSON.

THE STRONG AND THE WEAK. By Dora Peyser. (Currawong Publishing Co., Sydney, 1951. 131 pp.) Price (Aust.), 12s. 6d.

The theses advanced in this brief sociological survey of "assistance" may be summarised as follows:

Assistance is a universal feature of human societies, and helps to hold them together: every society has some regular forms of provision for those who are weak in various ways, children, the aged, the sick, the poor, and those cut off from the normal supporting group. Assistance always rests upon some other social bond or relationship, and "the obligation, urgency and quality of assistance increase with social nearness and decrease with social remoteness". There is a tendency, particularly when assistance itself becomes the major part of a relationship, for it to put the giver in a position of superiority and the receiver in one of inferiority; but in addition to asymmetrical relationships, there are "self-help groups" in which those similarly afflicted help one another. Different social groups, such as the primitive society, the family, the church, and the state, have their own characteristic forms of assistance, their respective strengths and weaknesses, so that each is more appropriate to some problems than to others; for example, the state can work only in a highly organised way, and tends to miss the special needs of individuals.

These points are developed and supported by a semi-historical survey which culminates in an account of recent attempts at assistance in the international sphere. In the course of this survey a number of important distinctions are drawn, e.g. between the kind of public assistance which takes over where the family breaks down, and the kind which helps the family to carry out its own typical forms of assistance.

On the whole these points are fairly obvious, but in stating and illustrating them this book has the value of giving warnings against generalised assumptions that ignore them, such as the assumptions that competition is more natural than co-operation, that assistance is "a sentimental weakness . . . an invention of later, softer civilisations", or that the state can and should take over the whole task of providing for those in need.

This book is simply and effectively written and well produced: it is easy to read and its main points fix themselves in the reader's mind. Its faults are not so much of omission as of underemphasis. Conflicts and tensions are mentioned but insufficiently stressed; e.g., while it is pointed out that "international relief work is minimum assistance" (p. 108), this is explained by the "law of nearness and remoteness" whereas a main reason for the limitation is surely what is mentioned (on p. 109) but not directly related to this, that anything more than minimum assistance runs the risk of aiding a competing or potentially hostile group. It is over-optimistic to state as a *description* of medical and hospital services that "organisation stops at the individuality of the patient" (pp. 68-9). Again the undesirable effects of the "downhill" nature of assistance (e.g. on the character of many social workers) are under-stressed. And in general there could have been consideration of the ethical character of the various patterns of assistance, of the different moralities that go along with mutual aid, the extended family system, charity, and the contributory and non-contributory forms of state security systems. It is significant that Dr. Peyser both mistranslates and misunderstands Rousseau's comment on the cosmopolitan *morality*. He was not criticising actual international aid but pointing out that there is a *spurious* "love of mankind" which cloaks a real coldness.

J. L. MACKIE.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

RELIGION, PHILOSOPHY AND PSYCHICAL RESEARCH. By C. D. Broad. (Routledge, 1953. 308 pp.) Price (U.K.), 25s.

A further selection of Broad's papers.

THE PHILOSOPHY OF J. S. MILL. By R. P. Anschutz. (Clarendon Press, 1953. 184 pp.) Price (Aust.), 28s. 3d.

THE METHOD OF DESCARTES—A STUDY OF THE *Regulae*. By L. J. Beck. (Clarendon Press, 1952. x + 316 pp.) Price (Aust.), 55s. 6d.

INTRODUCTION TO LOGIC. By Irving M. Copi. (Macmillan, 1953. xvi + 472 pp.) Price, $4.00.

A textbook introducing the student both to traditional and to symbolic logic, and concluding with a discussion of induction.

INQUIRIES INTO THE NATURE OF LAW AND MORALS. By Axel Hägerström, Edited by Karl Olivecrona, Translated by C. D. Broad. (Almqvist & Wiksell, Stockholm, 1953. xxxii + 377 pp.) Price, Sw.cr. 25.00.

SCIENCE AND AESTHETIC JUDGMENT—A STUDY IN TAINE'S CRITICAL METHOD. By Sholom J. Kahn. (Routledge, 1953. xii + 283 pp.) Price (U.K.), 25s.

PLOTINUS—A VOLUME OF SELECTIONS IN A NEW ENGLISH TRANSLATION. By A. H. Armstrong. (Allen & Unwin, 1953. 174 pp.) Price (U.K.), 10s. 6d.

PHILOSOPHY, AN INTRODUCTION. By Archie J. Bahm. (John Wiley & Sons, Inc., 1953. xiv + 441 pp.) Price, $4.50.

An introduction to the major problems of philosophy and the main types of solutions.

A HISTORY OF CHINESE PHILOSOPHY. VOL I: THE PERIOD OF THE PHILOSOPHERS. By Fung Yu-lan, Translated by Derk Bodde. (Allen & Unwin, 1953. xxxiv + 455 pp.) Price (U.K.), 40s.

HISTORY OF PHILOSOPHY: EASTERN AND WESTERN. Editorial board under the chairmanship of Radhakrishnan. (Allen & Unwin, 1953. Vol. I 617 pp., Vol. II 462 pp.) Price (U.K.), two volumes, 65s.

GEOGRAPHICAL HISTORY IN GREEK LANDS. By John L. Myres. (Clarendon Press, 1953. ix + 381 pp.) Price (Aust.), 63s.

A collection of papers written at various times.

TRAGEDY AND THE PARADOX OF THE FORTUNATE FALL. By Herbert Weisinger. (Routledge, 1953. 300 pp.) Price (U.K.), 21s.

THE IMPRISONED SPLENDOUR. By Raynor C. Johnson. (Hodder and Stoughton, 1953. 424 pp.) Price (U.K.), 25s.

The author, a physicist, argues from natural science, psychical research, and mystical experience to a spiritual view of reality.

THE ORIGIN AND GOAL OF HISTORY. By Karl Jaspers. (Routledge, 1953. xvi + 294 pp.) Price (U.K.), 21s.

THE PHILOSOPHY OF MAHATMA GANDHI. By D. M. Datta. (University of Wisconsin Press, 1953. xiv + 155 pp.) Price, $2.50.

SELECTION I. Edited by Cecily Hastings & Donald Nicholl. (Sheed & Ward, 1953. xxi + 216 pp.) Price (U.K.), 15s.

A collection of papers by various authors, on subjects ranging from the origin of man to the choice between Aristotelian and modern logic and the reign of Antichrist.

SOCIAL SERVICE AND MENTAL HEALTH. By Margaret Ashdown and S. Clement Brown. (Routledge, 1953. vii + 260 pp.) Price (U.K.), 16s.

LECTURES ON THE SCIENTIFIC BASIS OF MEDICINE. Vol. I 1951-52. British Postgraduate Medical Federation. (University of London, 1953. viii + 396 pp.) Price (U.K.), 30s.

AN INTRODUCTION TO MALTHUS. Edited by D. V. Glass. (C. A. Watts, 1953. x + 205 pp.) Price (U.K.), 10s. 6d.

Two works of Malthus, with three introductory essays.

ALICE IN BIBLELAND. By George Wils. (Philosophical Library, 1953. 54 pp.) Price, $2.75.

A series of conversations dealing with a child's perplexities in reading the Bible.

ATOMS, MEN AND GOD. By Paul E. Sabine. (Phil. Lib., 1953. x + 226 pp.) Price, $3.75.

PHILOSOPHY AND THE IDEOLOGICAL CONFLICT. By Charles S. Seely. (Phil. Lib., 1953. 319 pp.) Price, $5.00.

THE UNIVERSE OF MEANING. By Samuel Reiss. (Phil. Lib., 1953. x + 227 pp.) Price, $3.75.

ESSAY IN POLITICS. By Scott Buchanan. (Phil. Lib., 1953. xiii + 236 pp.) Price, $3.75.

FAITH AND MORAL AUTHORITY. By Ben Kimpel. (Phil. Lib., 1953. 186 pp.) Price, $2.75.

BUDDHISM AND ZEN. By Ruth S. McCandless and Nyogen Senzaki. (Phil. Lib., 1953. 91 pp.) Price, $3.75.

THE DOUBTING THOMAS TODAY. By Russell P. Davies. (Phil. Lib., 1953. 344 pp.) Price, $4.75.